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British Association for the Advancement  
of Science.

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MANCHESTER MEETING, 1887.

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# British Association for the Advancement of Science.

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MANCHESTER, 1887.

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## ADDRESS

BY

SIR HENRY E. ROSCOE,

M.P., D.C.L., LL.D., PH.D., F.R.S., V.P.C.S.,

PRESIDENT.

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MANCHESTER, distinguished as the birthplace of two of the greatest discoveries of modern science, heartily welcomes to-day for the third time the members and friends of the British Association for the Advancement of Science.

On the occasion of our first meeting in this city in the year 1842 the President, Lord Francis Egerton, commenced his address with a touching allusion to the veteran of science, John Dalton, the great chemist, the discoverer of the laws of chemical combination, the framer of the atomic theory upon which the modern science of chemistry may truly be said to be based. Lord Francis Egerton said: 'Manchester is still the residence of one whose name is uttered with respect wherever science is cultivated, who is here to-night to enjoy the honours due to a long career of persevering devotion to knowledge, and to receive from myself, if he will condescend to do so, the expression of my own deep personal regret that increase of years, which to him up to this hour has been but increase of wisdom, should have rendered him in respect of mere bodily strength unable to fill on this occasion an office which in his case would have received more honour than it could confer. I do regret that any cause should have prevented the present meeting in his native town from being associated with the name'—and here I must ask you to allow me to exchange the name of Dalton in 1842 for that of Joule in 1887, and to add again in the words of the President of the former year that I would gladly have served as a doorkeeper in any house where Joule, the father of science in Manchester, was enjoying his just pre-eminence.

For it is indeed true that the mantle of John Dalton has fallen on the shoulders of one well worthy to wear it, one to whom science owes a debt

of gratitude not less than that which it willingly pays to the memory of the originator of the atomic theory. James Prescott Joule it was who, in his determination of the mechanical equivalent of heat, about the very year of our first Manchester meeting, gave to the world of science the results of experiments which placed beyond reach of doubt or cavil the greatest and most far-reaching scientific principle of modern times, namely, that of the conservation of energy. This, to use the words of Tyndall, is indeed a generalisation of conspicuous grandeur fit to take rank with the principle of gravitation, more momentous, if that be possible, combining as it does the energies of the material universe into an organic whole, and enabling the eye of science to follow the flying shuttles of the universal power as it weaves what the Erdgeist in 'Faust' calls 'the living garment of God.'

It is well, therefore, for us to remember, in the midst of the turmoil of our active industrial and commercial life, that Manchester not only well represents the energy of England in these practical directions, but that it possesses even higher claims to our regard and respect as being the seat of discoveries of which the value not only to pure science is momentous, but which also lie at the foundation of all our material progress and all our industrial success. For without a knowledge of the laws of chemical combination all the marvellous results with which modern industrial chemistry has astonished the world could not have been achieved, whilst the knowledge of the quantitative relations existing between the several forms of energy, and the possibility of expressing their amount in terms of ordinary mechanics, are matters which now constitute the life-breath of every branch of applied science. For example, before Dalton's discovery every manufacturer of oil of vitriol—a substance now made each week in thousands of tons within a few miles of this spot—every manufacturer had his own notions of the quantity of sulphur which he ought to burn in order to make a certain weight of sulphuric acid, but he had no idea that only a given weight of sulphur can unite with a certain quantity of oxygen and of water to form the acid, and that an excess of any one of the component parts was not only useless but harmful. Thus, and in tens of thousands of other instances, Dalton replaced rule of thumb by scientific principle. In like manner the applications of Joule's determination of the mechanical equivalent of heat are even more general; the increase and measurement of the efficiency of our steam engines and the power of our dynamos are only two of the numerous examples which might be adduced of the practical value of Joule's work.

If the place calls up these thoughts, the time of our meeting also awakens memories of no less interest, in the recollection that we this year celebrate the Jubilee of her Most Gracious Majesty's accession to the throne. It is right that the members of the British Association for the Advancement of Science should do so with heart and voice, for although science requires and demands no royal patronage, we thereby express the feeling which must be uppermost in the hearts of all men of



science, the feeling of thankfulness that we have lived in an age which has witnessed an advance in our knowledge of nature, and a consequent improvement in the physical, and let us trust also in the moral and intellectual, well being of the people hitherto unknown; an age with which the name of Victoria will ever be associated.

To give even a sketch of this progress, to trace even in the merest outline the salient points of the general history of science during the fifty momentous years of her Majesty's reign, is a task far beyond my limited powers. It must suffice for me to point out to you, to the best of my ability, some few of the steps of that progress as evidenced in the one branch of science with which I am most familiar, and with which I am more closely concerned, the science of chemistry.

In the year 1837 chemistry was a very different science from that existing at the present moment. Priestley, it is true, had discovered oxygen, Lavoisier had placed the phenomena of combustion on their true basis, Davy had decomposed the alkalis, Faraday had liquefied many of the gases, Dalton had enunciated the laws of chemical combination by weight, and Gay Lussac had pointed out the fact that a simple volumetric relation governs the combination of the gases. But we then possessed no knowledge of chemical dynamics, we were then altogether unable to explain the meaning of the heat given off in the act of chemical combination. The atomic theory was indeed accepted, but we were as ignorant of the mode of action of the atoms and as incapable of explaining their mutual relationship as were the ancient Greek philosophers. Fifty years ago, too, the connection existing between the laws of life, vegetable and animal, and the phenomena of inorganic chemistry, was ill understood. The idea that the functions of living beings are controlled by the same forces, chemical and physical, which regulate the changes occurring in the inanimate world, was then one held by only a very few of the foremost thinkers of the time. Vital force was a term in everyone's mouth, an expression useful, as Goethe says, to disguise our ignorance, for

*Chemie  
Symb.*

Wo die Begriffe fehlen,  
Da tritt ein Wort zur rechten Zeit sich ein.

Indeed the pioneer of the chemistry of life, Liebig himself, cannot quite shake himself free from the bonds of orthodox opinion, and he who first placed the phenomena of life on a true basis cannot trust his chemical principles to conduct the affairs of the body, but makes an appeal to vital force to help him out of his difficulties; as when in the body politic an unruly mob requires the presence and action of physical force to restrain it and to bring its members under the saving influence of law and order, so too, according to Liebig's views, in the body corporeal a continual conflict between the chemical forces and the vital power occurs throughout life, in which the latter, when it prevails, insures health and a continuance of life, but of which defeat insures disease or death. The picture presented to the student of to-day is a very different one. We now believe that no

such conflict is possible, but that life is governed by chemical and physical forces, even though we cannot in every case explain its phenomena in terms of these forces; that whether these tend to continue or to end existence depends upon their nature and amount, and that disease and death are as much a consequence of the operation of chemical and physical laws as are health and life.

Looking back again to our point of departure fifty years ago, let us for a moment glance at Dalton's labours, and compare his views and those of his contemporaries with the ideas which now prevail. In the first place it is well to remember that the keystone of his atomic theory lies not so much in the idea of the existence and the indivisible nature of the particles of matter—though this idea was so firmly implanted in his mind that, being questioned on one occasion on the subject, he said to his friend the late Mr. Ransome, 'Thou knowst it must be so, for no man can split an atom'—as in the assumption that the weights of these particles are different. Thus whilst each of the ultimate particles of oxygen has the same weight as every other particle of oxygen, and each atom of hydrogen, for example, has the same weight as every other particle of hydrogen, the oxygen atom is sixteen times heavier than that of hydrogen, and so on for the atoms of every chemical element, each having its own special weight. It was this discovery of Dalton, together with the further one that the elements combine in the proportions indicated by the relative weights of their atoms or in multiples of these proportions, which at once changed chemistry from a qualitative to a quantitative science, making the old invocation prophetic, 'God created all things according to measure and to weight.'

The researches of chemists and physicists during the last fifty years have not only strengthened but broadened the foundations of the great Manchester philosopher's discoveries. It is true that his original numbers, obtained by crude and inaccurate methods, have been replaced by more exact figures, but his laws of combination and his atomic explanation of those laws stand as the great bulwarks of our science.

On the present occasion it is interesting to remember that within a stone's-throw of this place is the small room belonging to our Literary and Philosophical Society which served Dalton as his laboratory. Here with the simplest of all possible apparatus—a few cups, penny ink bottles, rough balances, and self-made thermometers and barometers—Dalton accomplished his great results. Here he patiently worked, marshalling facts to support his great theory, for as an explanation of his laborious experimental investigations the wise old man says: 'Having been in my progress so often misled, by taking for granted the results of others, I have determined to write as little as possible but what I can attest by my own experience.' Nor ought we when here assembled to forget that the last three of Dalton's experimental essays—one of which, on a new method of measuring water of crystallisation, contained more than the germ of a great discovery—were communicated to our Chemical Section



in 1842, and that this was the last memorable act of his scientific life. In this last of his contributions to science, as in his first, his method of procedure was that which has been marked out as the most fruitful by almost all the great searchers after nature's secrets, namely the assumption of a certain view as a working hypothesis, and the subsequent institution of experiment to bring this hypothesis to a test of reality upon which a legitimate theory is afterwards to be based. 'Dalton,' as Henry well says, 'valued detailed facts mainly, if not solely, as the stepping-stones to comprehensive generalisations.'

Next let us ask what light the research of the last fifty years has thrown on the subject of the Daltonian atoms: first, as regards their size; secondly, in respect to their indivisibility and mutual relationships; and thirdly, as regards their motions.

As regards the size and shape of the atoms, Dalton offered no opinion, for he had no experimental grounds on which to form it, believing that they were inconceivably small and altogether beyond the grasp of our senses aided by the most powerful appliances of art. He was in the habit of representing his atoms and their combinations diagrammatically as round discs or spheres made of wood, by means of which he was fond of illustrating his theory. But such mechanical illustrations are not without their danger, for I well remember the answer given by a pupil to a question on the atomic theory: 'Atoms are round balls of wood invented by Dr. Dalton.' So determinedly indeed did he adhere to his mechanical method of representing the chemical atoms and their combinations that he could not be prevailed upon to adopt the system of chemical formulæ introduced by Berzelius and now universally employed. In a letter addressed to Graham in April 1837 he writes: 'Berzelius' symbols are horrifying. A young student in chemistry might as soon learn Hebrew as make himself acquainted with them.' And again: 'They appear to me equally to perplex the adepts in science, to discourage the learner, as well as to cloud the beauty and simplicity of the atomic theory.'

But modern research has accomplished, as regards the size of the atom, at any rate to a certain extent, what Dalton regarded as impossible. Thus in 1865 Loschmidt, of Vienna, by a train of reasoning which I cannot now stop to explain, came to the conclusion that the diameter of an atom of oxygen or nitrogen was  $\frac{1}{10,000,000}$  of a centimetre. With the highest known magnifying power we can distinguish the  $\frac{1}{40,000}$  part of a centimetre; if now we imagine a cubic box each of whose sides has the above length, such a box when filled with air will contain from 60 to 100 millions of atoms of oxygen and nitrogen. A few years later William Thomson extended the methods of atomic measurement, and came to the conclusion that the distance between the centres of contiguous molecules is less than  $\frac{1}{5,000,000}$  and greater than  $\frac{1}{1,000,000,000}$  of a centimetre; or, to put it in language more suited to the ordinary mind, Thomson asks us to imagine a drop of water magnified up to the size of the earth, and then tells us that the coarseness of the graining of such a mass would be

Centimetre  
1  
10,000,000

something between a heap of small shot and a heap of cricket balls. Or again, to take Clifford's illustration, you know that our best microscopes magnify from 6,000 to 8,000 times; a microscope which would magnify that result as much again would show the molecular structure of water. Or again, to put it in another form, if we suppose that the minutest organism we can now see were provided with equally powerful microscopes, these beings would be able to see the atoms.

Next, as to the indivisibility of the atom, involving also the question as to the relationships between the atomic weights and properties of the several elementary bodies.

Taking Dalton's aphorism, 'Thou knowst no man can split an atom,' as expressing the view of the enunciator of the atomic theory, let us see how far this idea is borne out by subsequent work. In the first place, Thomas Thomson, the first exponent of Dalton's generalisation, was torn by conflicting beliefs until he found peace in the hypothesis of Prout, that the atomic weights of all the so-called elements are multiples of a common unit, which doctrine he sought to establish, as Thorpe remarks, by some of the very worst quantitative determinations to be found in chemical literature, though here I may add that they were not so incorrect as Dalton's original numbers.

Coming down to a somewhat later date, Graham, whose life was devoted to finding what the motion of an atom was, freed himself from the bondage of the Daltonian aphorism, and defined the atom not as a thing which cannot be divided, but as one which has not been divided. With him, as with Lucretius, as Angus Smith remarks, the original atom may be far down.

But speculative ideas respecting the constitution of matter have been the scientific relaxation of many minds from olden time to the present. In the mind of the early Greek the action of the atom as one substance taking various forms by unlimited combinations was sufficient to account for all the phenomena of the world. And Dalton himself, though upholding the indivisibility of his ultimate particles, says: 'We do not know that any of the bodies denominated elementary are absolutely indecomposable.' Again Boyle, treating of the origin of form and quality, says: 'There is one universal matter common to all bodies—an extended divisible and impenetrable substance.' Then Graham in another place expresses a similar thought when he writes: 'It is conceivable that the various kinds of matter now recognised as different elementary substances may possess one and the same ultimate or atomic molecules existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies.'

What experimental evidence is now before us bearing upon these interesting speculations? In the first place, then, the space of fifty years has completely changed the face of the inquiry. Not only has the number of distinct well-established elementary bodies increased from fifty-three in



1837 to seventy in 1887 (not including the *twenty* or more new elements recently said to have been discovered by Krüss and Nilson in certain rare Scandinavian minerals), but the properties of these elements have been studied, and are now known to us with a degree of precision then undreamt of. So that relationships existing between these bodies which fifty years ago were undiscernible are now clearly manifest, and it is to these relationships that I would for a moment ask your attention. I have already stated that Dalton measured the relative weights of the ultimate particles by assuming hydrogen as the unit, and that Prout believed that on this basis the atomic weights of all the other elements would be found to be multiples of the atomic weight of hydrogen, thus indicating that an intimate constitutional relation exists between hydrogen and all the other elements.

Since the days of Dalton and Prout the truth or otherwise of Prout's law has been keenly contested by the most eminent chemists of all countries. The inquiry is a purely experimental one, and only those who have a special knowledge of the difficulties which surround such inquiries can form an idea of the amount of labour and self-sacrifice borne by such men as Dumas, Stas, and Marignac in carrying out delicate researches on the atomic weights of the elements. What is, then, the result of these most laborious experiments? It is that, whilst the atomic weights of the elements are not exactly either multiples of the unit or of half the unit, many of the numbers expressing most accurately the weight of the atom approximate so closely to a multiple of that of hydrogen that we are constrained to admit that these approximations cannot be a mere matter of chance, but that some reason must exist for them. What that reason is, and why a close approximation and yet something short of absolute identity exists, is as yet hidden behind the veil; but who is there that doubts that when this Association celebrates its centenary this veil will have been lifted and this occult but fundamental question of atomic philosophy shall have been brought into the clear light of day?

But these are by no means all the relationships which modern science has discovered with respect to the atoms of our chemical elements. So long ago as 1829 Döbereiner pointed out that certain groups of elements exist presenting in all their properties strongly marked family characteristics, and this was afterwards extended and insisted upon by Dumas. We find, for example, in the well-known group of chlorine, bromine, and iodine, these resemblances well developed, accompanied moreover by a proportional graduation in their chemical and physical properties. Thus, to take the most important of all their characters, the atomic weight of the middle term is the mean of the atomic weights of the two extremes. But these groups of triads appeared to be unconnected in any way with one another, nor did they seem to bear any relation to the far larger number of the elements not exhibiting these peculiarities.

Things remained in this condition until 1863, when Newlands threw fresh light upon the subject, showing a far-reaching series of relation-

ships. For the first time we thus obtained a glance into the mode in which the elements are connected together, but, like so many new discoveries, this did not meet with the recognition which we now see it deserves. But whilst England thus had the honour of first opening up this new path, it is to Germany and to Russia that we must look for the consummation of the idea. Germany, in the person of Lothar Meyer, keeps, as it is wont to do, strictly within the limits of known facts. Russia, in the person of Mendelejeff, being of a somewhat more imaginative nature, not only seizes the facts which are proved, but ventures upon prophecy. These chemists, amongst whom Carnelley must be named, agree in placing all the elementary bodies in a certain regular sequence, thus bringing to light a periodic recurrence of analogous chemical and physical properties, on account of which the arrangement is termed the periodic system of the elements.

In order to endeavour to render this somewhat complicated matter clear to you, I may perhaps be allowed to employ a simile. Let us, if you please, imagine a series of human families, a French one, represented by Dumas, an English one, by name Newlands, a German one, the family of Lothar Meyer, and lastly a Russian one, that of Mendelejeff. Let us next imagine the names of these chemists placed in a horizontal line in the order I have mentioned. Then let us write under each the name of his father, and again, in the next lower line, that of his grandfather, followed by that of his great-grandfather, and so on. Let us next write against each of these names the number of years which has elapsed since the birth of the individual. We shall then find that these numbers regularly increase by a definite amount, *i.e.*, by the average age of a generation, which will be approximately the same in all the four families. Comparing the ages of the chemists themselves we shall observe certain differences, but these are small in comparison with the period which has elapsed since the birth of any of their ancestors. Now each individual in this series of family trees represents a chemical element; and just as each family is distinguished by certain idiosyncrasies, so each group of the elementary bodies thus arranged shows distinct signs of consanguinity.

But more than this, it not unfrequently happens that the history and peculiarities of some member of a family may have been lost, even if the memory of a more remote and more famous ancestor may be preserved, although it is clear that such an individual must have had an existence. In such a case Francis Galton would not hesitate from the characteristics of the other members to reproduce the physical and even the mental peculiarities of the missing member; and should genealogical research bring to light the true personal appearance and mental qualities of the man, these would be found to coincide with Galton's estimate.

Such predictions and such verifications have been made in the case of no less than three of our chemical elements. Thus, Mendelejeff pointed out that if, in the future, certain lacunæ in his table were to be filled, they must be filled by elements possessing chemical and physical pro-



perties which he accurately specified. Since that time these gaps have actually been stopped by the discovery of Gallium by Lecoq de Boisboudron, of Scandium by Nilson, and of Germanium by Winkler, and their properties, both physical and chemical, as determined by their discoverers, agree absolutely with those predicted by the Russian chemist. Nay, more than this, we not unfrequently have had to deal with chemical foundlings, elements whose parentage is quite unknown to us. A careful examination of the personality of such waifs has enabled us to restore them to the family from which they have been separated by an unkind fate, and to give them that position in chemical society to which they are entitled.

These remarkable results, though they by no means furnish a proof of the supposition already referred to, viz., that the elements are derived from a common source, clearly point in this direction, and lend some degree of colour to the speculations of those whose scientific imagination, wearying of dry facts, revels in picturing to itself an elemental Bathybius, and in applying to the inanimate, laws of evolution similar to those which rule the animate world. Nor is there wanting other evidence regarding this inquiry, for here heat, the great analyser, is brought into court. The main portion of the evidence consists in the fact that distinct chemical individuals capable of existence at low temperatures are incapable of existence at high ones, but split up into new materials possessing a less complicated structure than the original. And here it may be well to emphasise the distinction which the chemist draws between the atom and the molecule, the latter being a more or less complicated aggregation of atoms, and especially to point out the fundamental difference between the question of separating the atoms in the molecule and that of splitting up the atom itself. The decompositions above referred to are, in fact, not confined to compound bodies, for Victor Meyer has proved in the case of iodine that the molecule at high temperatures is broken to atoms, and J. J. Thomson has added to our knowledge by showing that this breaking up of the molecule may be effected, not only by heat vibrations, but likewise by the electrical discharge at a comparatively low temperature.

How far, now, has this process of simplification been carried? Have the atoms of our present elements been made to yield? To this a negative answer must undoubtedly be given, for even the highest of terrestrial temperatures, that of the electric spark, has failed to shake any one of these atoms in two. That this is the case has been shown by the results with which spectrum analysis, that new and fascinating branch of science, has enriched our knowledge, for that spectrum analysis does give us most valuable aid in determining the varying molecular conditions of matter is admitted by all. Let us see how this bears on the question of the decomposition of the elements, and let us suppose for a moment that certain of our present elements, instead of being distinct substances, were made up of common ingredients, and that these compound elements, if I may be allowed to use so incongruous a term, are split up at the temperature of the electric spark into less complicated molecules. Then

the spectroscopic examination of such a body must indicate the existence of these common ingredients by the appearance in the spark spectra of these elements of identical bright lines. Coincidences of this kind have indeed been observed, but on careful examination these have been shown to be due either to the presence of some one of the other elements as an impurity or to insufficient observational power. This absence of coincident lines admits, however, of two explanations—either that the elements are not decomposed at the temperature of the electric spark, or, what appears to me a much more improbable supposition, each one of the numbers of bright lines exhibited by every element indicates the existence of a separate constituent, no two of this enormous number being identical.

Terrestrial analysis having thus failed to furnish favourable evidence, we are compelled to see if any information is forthcoming from the chemistry of the sun and stars. And here I would remark that it is not my purpose now to dilate on the wonders which this branch of modern science has revealed. It is sufficient to remind you that chemists thus have the means placed at their disposal of ascertaining with certainty the presence of elements well known on this earth in fixed stars so far distant that we are now receiving the light which emanated from them perhaps even thousands of years ago.

Since Bunsen and Kirchhoff's original discovery in 1859, the labours of many men of science of all countries have largely increased our knowledge of the chemical constitution of the sun and stars, and to no one does science owe more in this direction than to Lockyer and Huggins in this country, and to Young in the New England beyond the seas. Lockyer has of late years devoted his attention chiefly to the varying nature of the bright lines seen under different conditions of time and place on the solar surface, and from these observations he has drawn the inference that the matching observed by Kirchhoff between, for instance, the iron lines as seen in our laboratories and those visible in the sun, has fallen to the ground. He further explains this want of uniformity by the fact that at the higher transcendental temperatures of the sun the substance which we know here as iron is resolved into separate components. Other experimentalists, however, while accepting Lockyer's facts as to the variations in the solar spectrum, do not admit his conclusions, and would rather explain the phenomena by the well-known differences which occur in the spectra of all the elements when their molecules are subject to change of temperature or change of position.

Further, arguments in favour of this idea of the evolution of the elements have been adduced from the phenomena presented by the spectra of the fixed stars. It is well known that some of these shine with a white, others with a red, and others again with a blue light; and the spectroscope, especially under the hands of Huggins, has shown that the chemical constitution of these stars is different. The white stars, of which Sirius may be taken as a type, exhibit a much less complicated spectrum than the orange and the red stars; the spectra of the latter



remind us more of those of the metalloids and of chemical compounds than of the metals. Hence it has been argued that in the white, presumably the hottest, stars a celestial dissociation of our terrestrial elements may have taken place, whilst in the cooler stars, probably the red, combination even may occur. But even in the white stars we have no *direct* evidence that a decomposition of any terrestrial atom has taken place; indeed we learn that the hydrogen atom, as we know it here, can endure unscathed the inconceivably fierce temperature of stars presumably many times more fervent than our sun, as Sirius and Vega.

Taking all these matters into consideration, we need not be surprised if the earthbound chemist should, in the absence of celestial evidence which is incontestable, continue, for the present at least, and until fresh evidence is forthcoming, to regard the elements as the unalterable foundation stones upon which his science is based.

Pursuing another line of inquiry on this subject, Crookes has added a remarkable contribution to the question of the possibility of decomposing the elements. With his well-known experimental prowess, he has discovered a new and beautiful series of phenomena, and has shown that the phosphorescent lights emitted by certain chemical compounds, especially the rare earths, under an electric discharge in a high vacuum exhibit peculiar and characteristic lines. For the purpose of obtaining his material Crookes started from a substance believed by chemists to be homogeneous, such, for example, as the rare earth yttria, and succeeded by a long series of fractional precipitations in obtaining products which yield different phosphorescent spectra, although when tested by the ordinary methods of what we may term high temperature spectroscopy, they appear to be the one substance employed at the starting point. The other touchstone by which the identity, or otherwise, of these various products might be ascertained, viz., the determination of their atomic weights, has not, as yet, engaged Crookes' attention. In explanation of these singular phenomena, the discoverer suggests two possibilities. First, that the bodies yielding the different phosphorescent spectra are different elementary constituents of the substance which we call yttria. Or, if this be objected to because they all yield the same spark spectrum, he adopts the very reasonable view that the Daltonian atom is probably, as we have seen, a system of chemical complexity; and adds to this the idea that these complex atoms are not all of exactly the same constitution and weight, the differences, however, being so slight that their detection has hitherto eluded our most delicate tests, with the exception of this one of phosphorescence in a vacuum. To these two explanations, Marignac, in a discussion of Crookes' results, adds a third. It having been shown by Crookes himself that the presence of the minutest traces of foreign bodies produce remarkable alterations in the phosphorescent spectra, Marignac suggests that in the course of the thousands of separations which must be made before these differences become manifest, traces of foreign bodies may have been accidentally introduced, or, being present

in the original material, may have accumulated to a different extent in the various fractions, their presence being indicated by the only test by which they can now be detected. Which of these three explanations is the true one must be left to future experiment to decide.

We must now pass from the statics to the dynamics of chemistry; that is, from the consideration of the atoms at rest to that of the atoms in motion. Here again we are indebted to John Dalton for the first step in this direction, for he showed that the particles of a gas are constantly flying about in all directions; that is, that gases diffuse into one another, as an escape of coal gas from a burner, for example, soon makes itself perceptible throughout the room. Dalton, whose mind was constantly engaged in studying the molecular condition of gases, first showed that a light gas cannot rest upon a heavier gas as oil upon water, but that an interpenetration of each gas by the other takes place. It is, however, to Graham's experiments, made rather more than half a century ago, that we are indebted for the discovery of the law regulating these molecular motions of gases, proving that their relative rates of diffusion are inversely proportional to the square roots of their densities, so that oxygen being 16 times heavier than hydrogen, their relative rates of diffusion are 1 and 4.

But whilst Dalton and Graham indicated that the atoms are in a continual state of motion, it is to Joule that we owe the first accurate determination of the rate of that motion. At the Swansea Meeting in 1848, Joule read a paper before Section A on the Mechanical Equivalent of Heat and on the Constitution of Elastic Fluids. In this paper Joule remarks that whether we conceive the particles to be revolving round one another according to the hypothesis of Davy, or flying about in every direction according to Herapath's view, the pressure of the gas will be in proportion to the *vis viva* of its particles. 'Thus it may be shown that the particles of hydrogen at the barometrical pressure of 30 inches at a temperature of 60° must move with a velocity of 6225·54 feet per second in order to produce a pressure of 14·714 lbs. on the square inch;' or, to put it in other words, a molecular cannonade or hailstorm of particles, at the above rate—a rate, we must remember, far exceeding that of a cannon ball—is maintained against the bounding surface.

We can, however, go a step further and calculate with Clerk Maxwell the number of times in which this hydrogen molecule, moving at the rate of 70 miles per minute, strikes against others of the vibrating swarm, and we learn that in one second of time it must knock against others no less than 18 thousand million times.

And here we may pause and dwell for a moment on the reflection that in nature there is no such thing as great or small, and that the structure of the smallest particle, invisible even to our most searching vision, may be as complicated as that of any one of the heavenly bodies which circle round our sun.

But how does this wonderful atomic motion affect our chemistry? Can chemical science or chemical phenomena throw light upon this



motion, or can this motion explain any of the known phenomena of our science? I have already said that Lavoisier left untouched the dynamics of combustion. He could not explain why a fixed and unalterable amount of heat is in most cases emitted but in some cases absorbed when chemical combination takes place. What Lavoisier left unexplained Joule has made clear. On August 25, 1843, Joule read a short communication, I am glad to remember, before the Chemical Section of our Association, meeting that year at Cork, containing an announcement of a discovery which was to revolutionise modern science. This consisted in the determination of the mechanical equivalent of heat, in proving by accurate experiment that the expenditure of energy equal to that developed by the weight of 772 pounds falling through one foot at Manchester, the temperature of one pound of water can be raised  $1^{\circ}$  Fahrenheit. In other words, every change in the arrangement of the particles is accompanied by a definite evolution or an absorption of heat. In all such cases the molecular energy leaves the potential to assume the kinetic form, or *vice versâ*. Heat is evolved by the clashing of the atoms, and this amount is fixed and definite.

Thus it is to Joule we owe the foundation of chemical dynamics and the basis of thermal chemistry. As the conservation of mass or the principle of the indestructibility of matter forms the basis of chemical statics, so the principle of the conservation of energy<sup>1</sup> constitutes the foundation of chemical dynamics. Change in the form of matter and change in the form of energy are the universal accompaniments of every chemical operation. Here again it is to Joule we owe the proof of the truth of this principle in another direction, viz., that when electrical energy is developed by chemical change a corresponding quantity of chemical energy disappears. Energy as defined by Maxwell is the power of doing work, and work is the act of producing a change of configuration in a system in opposition to a force which resists that change. Chemical action produces such a change of configuration in the molecules. Hence, as Maxwell says, 'a complete knowledge of the mode in which the potential energy of a system varies with the configuration would enable us to predict every possible motion of the system under the action of given external forces, provided we were able to overcome the purely mathematical difficulties of the calculation.' The object of thermal chemistry is to measure these changes of energy by thermal methods, and to connect these with chemical changes, to estimate the attractions of the atoms and molecules to which the name of chemical affinity has been applied, and thus to solve the most fundamental problem of chemical science. How far has modern research approached the solution of this most difficult problem? How far can we answer the question,

<sup>1</sup> 'The total energy of any material system is a quantity which can neither be increased nor diminished by any action between the parts of the system, though it may be transformed into any of the forms of which energy is susceptible.'—MAXWELL.

what is the amount of the forces at work in these chemical changes? What laws govern these forces? Well, even in spite of the results with which recent researches, especially the remarkable ones of the Danish philosopher Thomsen have enriched us, we must acknowledge that we are yet scarcely in sight of Maxwell's position of successful prediction. Thermal chemistry, we must acknowledge, is even yet in its infancy; it is, however, an infant of sturdy growth, likely to do good work in the world, and to be a credit to him who is its acknowledged father, as well as to those who have so carefully tended it in its early years.

Chemistry  
But recent investigation in another direction bids fair even to eclipse the results which have been obtained by the examination of thermal phenomena. And this lies in the region of electrical chemistry. Faraday's work relating to conductivity of chemical substances has been already referred to, and this has been since substantiated and extended to pure substances by Kohlrausch. It has been shown, for example, that the resistance of absolutely pure water is almost an infinite quantity. But a small quantity of an acid, such as acetic or butyric acid, greatly increases the conductivity; but more than this, it is possible by determination of the conductivity of a mixture of water with these two acids to arrive at a conclusion as to the partition of the molecules of the water between the acids. Such a partition, however, implies a change of position, and therefore we are furnished with a means of recognising the motion of the molecules in a liquid, and of determining its amount. Thus it has been found that the hindrance to molecular motion is more affected by the chemical character of the liquid than by physical characters such as viscosity. We have seen that chemical change is always accompanied by molecular motion, and further evidence of the truth of this is gained from the extraordinary chemical inactivity of pure unmixed substances. Thus pure anhydrous hydrochloric acid does not act upon lime, whereas the addition of even a trace of moisture sets up a most active chemical change, and hundreds of other examples of a similar kind might be stated. Bearing in mind that these pure anhydrous compounds do not conduct, we are led to the conclusion that an intimate relation exists between chemical activity and conductivity. And we need not stop here; for a method is indicated indeed by which it will be possible to arrive at a measure of chemical affinity from determination of conductivity. It has indeed been already shown that the rate of change in the saponification of acetic ether is directly proportional to the conductivity of the liquid employed.

Such wide-reaching inquiries into new and fertile fields, in which we seem to come into nearer touch with the molecular state of matter, and within a measurable distance of accurate mathematical expression, leads to confident hope that Lord Rayleigh's pregnant words at Montreal may ere long be realised: 'It is from the further study of electrolysis that we may expect to gain improved views as to the nature of chemical reactions, and of the forces concerned in bringing them about; and I cannot help



thinking that the next great advance, of which we already have some foreshadowing, will come on this side.'

There is, perhaps, no branch of our science in which the doctrine of the Daltonian atom plays a more conspicuous part than in organic chemistry or the chemistry of the carbon compounds, as there is certainly none in which such wonderful progress has been made during the last fifty years. One of the most striking and perplexing discoveries made rather more than half a century ago was that chemical compounds could exist which, whilst possessing an identical chemical composition, that is containing the same percentage quantity of their constituents, are essentially distinct chemical substances exhibiting different properties. Dalton was the first to point out the existence of such substances, and to suggest that the difference was to be ascribed to a different or to a multiple arrangement of the constituent atoms. Faraday soon afterwards proved that this supposition was correct, and the research of Liebig and Wöhler on the identity of composition of the salts of fulminic and cyanic acid gave further confirmation to the conclusion, leading Faraday to remark that 'now we are taught to look for bodies composed of the same elements in the same proportion but differing in their qualities, they may probably multiply upon us.' How true this prophecy has become we may gather from the fact that we now know of thousands of cases of this kind, and that we are able not only to explain the reason of their difference by virtue of the varying position of the atoms within the molecule, but even to predict the number of distinct variations in which any given chemical compound can possibly exist. How large this number may become may be understood from the fact that, for example, one chemical compound, a hydrocarbon containing thirteen atoms of carbon combined with twenty-eight atoms of hydrogen, can be shown to be capable of existing in no less than 802 distinct forms.

Experiment in every case in which it has been applied has proved the truth of such a prediction, so that the chemist has no need to apply the cogent argument sometimes said to be used by experimentalists enamoured of pet theories, 'When facts do not agree with theory, so much the worse for the facts!' This power of successful prediction constitutes a high-water mark in science, for it indicates that the theory upon which such a power is based is a true one.

But if the Daltonian atom forms the foundation of this theory, it is upon a knowledge of the mode of arrangement of these atoms and on a recognition of their distinctive properties that the superstructure of modern organic chemistry rests. Certainly it does appear almost to verge on the miraculous that chemists should now be able to ascertain with certainty the relative position of atoms in a molecule so minute that millions upon millions, like the angels in the schoolmen's discussion, can stand on a needle's point. And yet this process of orientation is one which is accomplished every day in our laboratories, and one which more than any other has led to results of a startling character. Still, this

sword to open the oyster of science would have been wanting to us if we had not taken a step farther than Dalton did, in the recognition of the distinctive nature of the elemental atoms. We now assume on good grounds that the atom of each element possesses distinct capabilities of combination; some a single capability, others a double, others a triple, and others again a fourfold combining capacity. The germs of this theory of valency, one of the most fruitful of modern chemical ideas, were enunciated by Frankland in 1852, but the definite explanation of the linking of atoms, of the tetrad nature of the carbon atoms, their power of combination, and of the difference in structure between the fatty and aromatic series of compounds, was first pointed out by Kekulé in 1857; though we must not forget that this great principle was foreshadowed so long ago as 1833 from a physical point of view by Faraday in his well-known laws of electrolysis, and that it is to Helmholtz in his celebrated Faraday lecture that we owe the complete elucidation of the subject; for, whilst Faraday has shown that the number of the atoms electrolytically deposited is in the inverse ratio of their valencies, Helmholtz has explained this by the fact that the quantity of electricity with which each atom is associated is directly proportional to its valency.

Amongst the tetrad class of elements, carbon, the distinctive element of organic compounds, finds its place; and the remarkable fact that the number of carbon compounds far exceeds that of all the other elements put together receives its explanation. For these carbon atoms not only possess four means of grasping other atoms, but these four-handed carbon atoms have a strong partiality for each other's company, and readily attach themselves hand in hand to form open chains or closed rings to which the atoms of other elements join to grasp the unoccupied carbon hand, and thus to yield a dancing company in which all hands are locked together. Such a group, each individual occupying a given position with reference to the others, constitutes the organic molecule. When, in such a company, the individual members change hands, a new combination is formed. And as in such an assembly the eye can follow the changing positions of the individual members, so the chemist can recognise in his molecule the position of the several atoms, and explain by this the fact that each arrangement constitutes a new chemical compound possessing different properties, and account in this way for the decompositions which each differently constituted molecule is found to undergo.

Chemists are, however, not content with representing the arrangement of the atoms in one plane, as on a sheet of paper, but attempt to express the position of the atoms in space. In this way it is possible to explain certain observed differences in isomeric bodies which otherwise baffled our efforts. To Van t'Hoff, in the first instance, and more recently to Wislicenus, chemistry is indebted for work in this direction, which throws light on hitherto obscure phenomena, and points the way to still further and more important advances.

It is this knowledge of the mode in which the atoms in the molecule



are arranged, this power of determining the nature of this arrangement, which has given to organic chemistry the impetus which has overcome so many experimental obstacles, and given rise to such unlooked-for results. Organic chemistry has now become synthetic. In 1837 we were able to build up but very few and very simple organic compounds from their elements; indeed the views of chemists were much divided as to the possibility of such a thing. Both Gmelin and Berzelius argued that organic compounds, unlike inorganic bodies, cannot be built up from their elements. Organic compounds were generally believed to be special products of the so-called vital force, and it was only intuitive minds, like those of Liebig and Wöhler, who foresaw what was coming, and wrote in 1837 strongly against this view, asserting that the artificial production in our laboratories of all organic substances, so far as they do not constitute a living organism, is not only probable but certain. Indeed, they went a step farther, and predicted that sugar, morphia, salicine, will all thus be prepared; a prophecy which, I need scarcely remind you, has been after fifty years fulfilled, for at the present time we can prepare an artificial sweetening principle, an artificial alkaloid, and salicine.

In spite of these predictions, and in spite of Wöhler's memorable discovery in 1828 of the artificial production of urea, which did in reality break down for ever the barrier of essential chemical difference between the products of the inanimate and of the animate world, still, even up to a much later date, contrary opinions were held, and the synthesis of urea was looked upon as the exception which proves the rule. So it came to pass that for many years the artificial production of any of the more complicated organic substances was believed to be impossible. Now the belief in a special vital force has disappeared like the *ignis fatuus*, and no longer lures us in the wrong direction. We know now that the same laws regulate the formation of chemical compounds in both animate and inanimate nature, and the chemist only asks for a knowledge of the constitution of any definite chemical compound found in the organic world in order to be able to promise to prepare it artificially.

But the progress of synthetic organic chemistry, which has of late been so rapid, was made in the early days of the half-century only by feeble steps and slow. Seventeen long years elapsed between Wöhler's discovery and the next real synthesis. This was accomplished by Kolbe, who in 1845 prepared acetic acid from its elements. But then a splendid harvest of results gathered in by chemists of all nations quickly followed, a harvest so rich and so varied that we are apt to be overpowered by its wealth, and amidst so much that is alluring and striking we may well find it difficult to choose the most appropriate examples for illustrating the power and the extent of modern chemical synthesis.

Next, as a contrast to our picture, let us for a moment glance back again to the state of things fifty years ago, and then notice the chief steps by which we have arrived at our present position. In 1837 organic chemistry possessed no scientific basis, and therefore no classification of a

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character worthy of the name. Writing to Berzelius in that year, Wöhler describes the condition of organic chemistry as one enough to drive a man mad. 'It seems to me,' says he, 'like the tropical forest primæval, full of the strangest growths, an endless and pathless thicket in which a man may well dread to wander.' Still clearances had already been made in this wilderness of facts. Berzelius in 1832 welcomed the results of Liebig and Wöhler's research on benzoic acid as the dawn of a new era; and such it really was, inasmuch as it introduced a novel and fruitful idea, namely the possibility of a group of atoms acting like an element by pointing out the existence of organic radicals. This theory was strengthened and confirmed by Bunsen's classical researches on the cacodyl compounds, in which he showed that a common group of elements which acts exactly as a metal can exist in the free state, and this was followed soon afterwards by isolation of the so-called alcohol radicals by Frankland and Kolbe. It is, however, to Schorlemmer that we owe our knowledge of the true constitution of these bodies, a matter which proved to be of vital importance for the further development of the science.

Turning our glance in another direction we find that Dumas, in 1834, by his law of substitution threw light upon a whole series of singular and unexplained phenomena by showing that an exchange can take place between the constituent atoms in a molecule. Laurent indeed went farther, and assumed that a chlorine atom, for example, took up the position vacated by an atom of hydrogen and played the part of its displaced rival, so that the chemical and physical properties of the substitution-product were thought to remain substantially the same as those of the original body. A singular story is connected with this discovery. At a soirée in the Tuileries in the time of Charles X. the guests were almost suffocated by acrid vapours which were evidently emitted by the burning wax candles, and the great chemist Dumas was called in to examine into the cause of the annoyance. He found that the wax of which the candles were made had been bleached by chlorine, that a replacement of some of the hydrogen atoms of the wax by chlorine had occurred, and that the suffocating vapours consisted of hydrochloric acid given off during the combustion. The wax was as white and as odourless as before, and the fact of the substitution of chlorine for hydrogen could only be recognised when the candles were destroyed by burning. This incident induced Dumas to investigate more closely this class of phenomena, and the results of this investigation are embodied in his law of substitution. So far indeed did the interest of the French school of chemists lead them that some assumed that not only the hydrogen but also the carbon of organic bodies could be replaced by substitution. Against this idea Liebig protested, and in a satirical vein he informs the chemical public, writing from Paris under the *nom de plume* of S. Windler, that he has succeeded in substituting not only the hydrogen but the oxygen and carbon in cotton cloth by chlorine, and he adds that the London shops are now selling nightcaps and other articles of apparel made entirely



of chlorine, goods which meet with much favour, especially for hospital use!

But the debt which chemistry, both inorganic and organic, thus owes to Dumas' law of substitution is serious enough, for it proved to be the germ of Williamson's classical researches on etherification, as well as of those of Wurtz and Hofmann on the compound ammonias, investigations which lie at the base of the structure of modern chemistry. Its influence has been, however, still more far-reaching, inasmuch as upon it depends in great measure the astounding progress made in the wide field of organic synthesis.

It may here be permitted to me to sketch in rough outline the principles upon which all organic syntheses have been effected. We have already seen that as soon as the chemical structure of a body has been ascertained its artificial preparation may be certainly anticipated, so that the first step to be taken is the study of the structure of the naturally occurring substance which it is desired to prepare artificially by resolving it into simpler constituents, the constitution of which is already known. In this way, for example, Hofmann discovered that the alkaloid coneine, the poisonous principle of hemlock, may be decomposed into a simpler substance well known to chemists under the name of pyridine. This fact having been established by Hofmann, and the grouping of the atoms approximately determined, it was then necessary to reverse the process, and, starting with pyridine, to build up a compound of the required constitution and properties, a result recently achieved by Ladenburg in a series of brilliant researches. The well-known synthesis of the colouring matter of madder by Graebe and Liebermann, preceded by the important researches of Schunck, and that of indigo by Baeyer, are other striking examples in which this method has been successfully followed.

Not only has this intimate acquaintance with the changes which occur within the molecules of organic compounds been utilised, as we have seen, in the synthesis of naturally occurring substances, but it has also led to the discovery of many new ones. Of these perhaps the most remarkable instance is the production of an artificial sweetening agent termed saccharin, 250 times sweeter than sugar, prepared by a complicated series of reactions from coal-tar. Nor must we imagine that these discoveries are of scientific interest only, for they have given rise to the industry of the coal-tar colours, the value of which is measured by millions sterling annually, an industry which Englishmen may be proud to remember was founded by our countryman Perkin.

Another interesting application of synthetic chemistry to the needs of everyday life is the discovery of a series of valuable febrifuges, amongst which I may mention antipyrin as the most useful. An important aspect in connection with the study of these bodies is the physiological value which has been found to attach to the introduction of certain organic radicals, so that an indication is given of the possibility

of preparing a compound which will possess certain desired physiological properties, or even to foretell the kind of action which such bodies may exert on the animal economy.

But it is not only the physiological properties of chemical compounds which stand in intimate relation with their constitution, for we find that this is the case with all their physical properties. It is true that at the beginning of our period any such relation was almost unsuspected, whilst at the present time the number of instances in which this connection has been ascertained is almost infinite. Amongst these perhaps the most striking is the relationship which has been pointed out between the optical properties and chemical composition. This was in the first place recognised by Pasteur in his classical researches on racemic and tartaric acids in 1848; but the first to indicate a quantitative relationship and a connection between chemical structure and optical properties was Gladstone in 1863. Great instrumental precision has been brought to bear on this question, and consequently most important practical applications have resulted. I need only refer to the well-known accurate methods now in everyday use for the determination of sugar by the polariscope, equally valuable to the physician and to the manufacturer.

But now the question may well be put, is any limit set to this synthetic power of the chemist? Although the danger of dogmatising as to the progress of science has already been shown in too many instances, yet one cannot help feeling that the barrier which exists between the organised and unorganised worlds is one which the chemist at present sees no chance of breaking down.

It is true that there are those who profess to foresee that the day will arrive when the chemist, by a succession of constructive efforts, may pass beyond albumen, and gather the elements of lifeless matter into a living structure. Whatever may be said regarding this from other standpoints, the chemist can only say that at present no such problem lies within his province. Protoplasm, with which the simplest manifestations of life are associated, is not a compound, but a structure built up of compounds. The chemist may successfully synthesise any of its component molecules, but he has no more reason to look forward to the synthetic production of the structure than to imagine that the synthesis of gallic acid leads to the artificial production of gall-nuts.

Although there is thus no prospect of our effecting a synthesis of organised material, yet the progress made in our knowledge of the chemistry of life during the last fifty years has been very great, and so much so indeed that the sciences of physiological and of pathological chemistry may be said to have entirely arisen within this period.

In the introductory portion of this address I have already referred to the relations supposed to exist fifty years ago between vital phenomena and those of the inorganic world. Let me now briefly trace a few of the more important steps which have marked the progress of this branch of science during this period. Certainly no portion of our science is of



greater interest, nor, I may add, of greater complexity, than that which, bearing on the vital functions both of plants and of animals, endeavours to unravel the tangled skein of the chemistry of life, and to explain the principles according to which our bodies live, and move, and have their being. If, therefore, in the less complicated problems with which other portions of our science have to deal, we find ourselves, as we have seen, often far from possessing satisfactory solutions, we cannot be surprised to learn that with regard to the chemistry of the living body—whether vegetable or animal—in health or disease we are still farther from a complete knowledge of phenomena, even those of fundamental importance.

It is of interest here to recall the fact that nearly fifty years ago Liebig presented to the Chemical Section of this Association a communication in which, for the first time, an attempt was made to explain the phenomena of life on chemical and physical lines, for in this paper he admits the applicability of the great principle of the conservation of energy to the functions of animals, pointing out that the animal cannot generate more heat than is produced by the combustion of the carbon and hydrogen of his food.

'The source of animal heat,' says Liebig, 'has previously been ascribed to nervous action or to the contraction of the muscles, or even to the mechanical motions of the body, as if these motions could exist without an expenditure of force [equal to that] consumed in producing them.' Again he compares the living body to a laboratory furnace in which a complicated series of changes occur in the fuel, but in which the end-products are carbonic acid and water, the amount of heat evolved being dependent, not upon the intermediate, but upon the final products. Liebig asked himself the question, does every kind of food go to the production of heat; or can we distinguish, on the one hand, between the kind of food which goes to create warmth, and, on the other, that by the oxidation of which the motions and mechanical energy of the body are kept up? He thought that he was able to do this, and he divided food into two categories; the starchy or carbohydrate food is that, said he, which by its combustion provides the warmth necessary for the existence and life of the body. The albuminous or nitrogenous constituents of our food, the flesh meat, the gluten, the casein out of which our muscles are built up, are not available for the purposes of creating warmth, but it is by the waste of those muscles that the mechanical energy, the activity, the motions of the animal are supplied. We see, said Liebig, that the Esquimaux feeds on fat and tallow, and this burning in his body keeps out the cold. The Gaucho, riding on the pampas, lives entirely on dried meat, and the rowing man and pugilist, trained on beefsteaks and porter, require little food to keep up the temperature of their bodies, but much to enable them to meet the demand for fresh muscular tissue, and for this purpose they need to live on a strongly nitrogenous diet.

Thus far Liebig. Now let us turn to the present state of our know-

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ledge. The question of the source of muscular power is one of the greatest interest, for, as Frankland observes, it is the corner-stone of the physiological edifice and the key to the nutrition of animals.

Let us examine by the light of modern science the truth of Liebig's view—even now not uncommonly held—as to the functions of the two kinds of food, and as to the cause of muscular exercise being the oxidation of the muscular tissue. Soon after the promulgation of these views, J. R. Mayer, whose name as the first expositor of the idea of the conservation of energy is so well known, warmly attacked them, throwing out the hypothesis that all muscular action is due to the combustion of food, and not to the destruction of muscle, proving his case by showing that if the muscles of the heart be destroyed in doing mechanical work the heart would be burnt up in eight days! What does modern research say to this question? Can it be brought to the crucial test of experiment? It can; but how? Well, in the first place we can ascertain the work done by a man or any other animal; we can measure this work in terms of our mechanical standard, in kilogramme-metres or foot-pounds. We can next determine what is the destruction of nitrogenous tissue at rest and under exercise by the amount of nitrogenous material thrown off by the body. And here we must remember that these tissues are never completely burnt, so that free nitrogen is never eliminated. If now we know the heat-value of the burnt muscle, it is easy to convert this into its mechanical equivalent, and thus measure the energy generated. What is the result? Is the weight of muscle destroyed by ascending the Faulhorn or by working on the treadmill sufficient to produce on combustion heat enough when transformed into mechanical exercise to lift the body up to the summit of the Faulhorn or to do the work on the treadmill? Careful experiment has shown that this is so far from being the case that the actual energy developed is twice as great as that which could possibly be produced by the oxidation of the nitrogenous constituents eliminated from the body during twenty-four hours. That is to say, taking the amount of nitrogenous substance cast off from the body, not only whilst the work was being done but during twenty-four hours, the mechanical effect capable of being produced by the muscular tissue from which this cast-off material is derived would only raise the body halfway up the Faulhorn, or enable the prisoner to work half his time on the treadmill.

Hence it is clear that Liebig's proposition is not true. The nitrogenous constituents of the food do doubtless go to repair the waste of muscle, which, like every other portion of the body, needs renewal, whilst the function of the non-nitrogenous food is not only to supply the animal heat, but also to furnish, by its oxidation, the muscular energy of the body.

We thus come to the conclusion that it is the potential energy of the food which furnishes the actual energy of the body, expressed in terms either of heat or of mechanical work.

But there is one other factor which comes into play in this question



of mechanical energy, and must be taken into account; and this factor we are as yet unable to estimate in our usual terms. It concerns the action of the mind upon the body, and, although incapable of exact expression, exerts none the less an important influence on the physics and chemistry of the body, so that a connection undoubtedly exists between intellectual activity or mental work and bodily nutrition. In proof that there is a marked difference between voluntary and involuntary work, we need only compare the mechanical action of the heart, which never causes fatigue, with that of the voluntary muscles, which become fatigued by continued exertion. So, too, we know well that an amount of drill which is fatiguing to the recruit is not felt by the old soldier, who goes through the evolutions automatically. What is the expenditure of mechanical energy which accompanies mental effort, is a question which science is probably far removed from answering. But that the body experiences exhaustion as the result of mental activity is a well-recognised fact. Indeed, whilst the second law of thermodynamics teaches that in none of the mechanical contrivances for the conversion of heat into actual energy can such a conversion be complete, it is perhaps possible, as Helmholtz has suggested, that such a complete conversion may take place in the subtle mechanism of the animal organism.

The phenomena of vegetation, no less than those of the animal world, have, however, during the last fifty years been placed by the chemist on an entirely new basis. Although before the publication of Liebig's celebrated report on chemistry and its application to agriculture, presented to the British Association in 1840, much had been done, many fundamental facts had been established, still Liebig's report marks an era in the progress of this branch of our science. He not only gathered up in a masterly fashion the results of previous workers, but put forward his own original views with a boldness and frequently with a sagacity which gave a vast stimulus and interest to the questions at issue. As a proof of this I may remind you of the attack which he made on, and the complete victory which he gained over, the humus theory. Although Saussure and others had already done much to destroy the basis of this theory, yet the fact remained that vegetable physiologists up to 1840 continued to hold to the opinion that humus, or decayed vegetable matter, was the only source of the carbon of vegetation. Liebig, giving due consideration to the labours of Saussure, came to the conclusion that it was absolutely impossible that the carbon deposited as vegetable tissue over a given area, as for instance over an area of forest land, could be derived from humus, which is itself the result of the decay of vegetable matter. He asserted that the whole of the carbon of vegetation is obtained from the atmospheric carbonic acid, which, though only present in the small relative proportion of 4 parts in 10,000 of air, is contained in such absolutely large quantity that if all the vegetation on the earth's surface were burnt, the proportion of carbonic acid which would thus be thrown into the air would not be sufficient to double the present amount.

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That this conclusion of Liebig's is correct needed experimental proof, but such proof could only be given by long-continued and laborious experiment, and this serves to show that chemical research is not now confined to laboratory experiments lasting perhaps a few minutes, but that it has invaded the domain of agriculture as well as of physiology, and reckons the periods of her observations in the field not by minutes, but by years. It is to our English agricultural chemists Lawes and Gilbert that we owe the complete experimental proof required. And it is true that this experiment was a long and tedious one, for it has taken forty-four years to give the definite reply. At Rothamsted a plot was set apart for the growth of wheat. For forty-four successive years that field has grown wheat without addition of any carbonised manure; so that the only possible source from which the plant could obtain the carbon for its growth is the atmospheric carbonic acid. Now, the quantity of carbon which on an average was removed in the form of wheat and straw from a plot manured only with mineral matter was 1,000 pounds, whilst on another plot, for which a nitrogenous manure was employed, 1,500 pounds more carbon was annually removed; or 2,500 pounds of carbon are removed by this crop annually without the addition of any carbonaceous manure. So that Liebig's prevision has received a complete experimental verification.

May I, without wearying you with experimental details, refer for a moment to Liebig's views as to the assimilation of nitrogen by plants—a much more complicated and difficult question than the one we have just considered—and compare these with the most modern results of agricultural chemistry? We find that in this case his views have not been substantiated. He imagined that the whole of the nitrogen required by the plant was derived from atmospheric ammonia; whereas Lawes and Gilbert have shown by experiments of a similar nature to those just described, and extending over a nearly equal length of time, that this source is wholly insufficient to account for the nitrogen removed in the crop, and have come to the conclusion that the nitrogen must have been obtained either from a store of nitrogenous material in the soil or by absorption of free nitrogen from the air. These two apparently contradictory alternatives may perhaps be reconciled by the recent observations of Warrington and of Berthelot, which have thrown light upon the changes which the so-called nitrogenous capital of the soil undergoes, as well as upon its chemical nature, for the latter has shown that under certain conditions the soil has the power of absorbing the nitrogen of the air, forming compounds which can subsequently be assimilated by the plant.

Touching us as human beings even still more closely than the foregoing, is the influence which chemistry has exerted on the science of pathology, and in no direction has greater progress been made than in the study of micro-organisms in relation to health and disease. In the complicated chemical changes to which we give the names of fermentation



and putrefaction, the views of Liebig, according to which these phenomena are of a purely chemical character, have given way under the searching investigations of Pasteur, who established the fundamental principle that these processes are inseparably connected with the life of certain low forms of organisms. Thus was founded the science of bacteriology which in Lister's hands has yielded such splendid results in the treatment of surgical cases; and in those of Klebs, Koch, and others, has been the means of detecting the cause of many diseases both in man and animals; the latest and not the least important of which is the remarkable series of successful researches by Pasteur into the nature and mode of cure of that most dreadful of maladies, hydrophobia. And here I may be allowed to refer with satisfaction to the results of the labours on this subject of a committee, the formation of which I had the honour of moving for in the House of Commons. These results confirm in every respect Pasteur's assertions, and prove beyond a doubt that the adoption of his method has prevented the occurrence of hydrophobia in a large proportion of persons bitten by rabid animals, who, if they had not been subjected to this treatment would have died of that disease. The value of his discovery is, however, greater than can be estimated by its present utility, for it shows that it may be possible to avert other diseases besides hydrophobia by the adoption of a somewhat similar method of investigation and of treatment. This, though the last, is certainly not the least of the debts which humanity owes to the great French experimentalist. Here it might seem as if we had outstepped the boundaries of chemistry, and have to do with phenomena purely vital. But recent research indicates that this is not the case, and points to the conclusion that the microscopist must again give way to the chemist, and that it is by chemical rather than by biological investigation that the causes of diseases will be discovered, and the power of removing them obtained. For we learn that the symptoms of infective diseases are no more due to the microbes which constitute the infection than alcoholic intoxication is produced by the yeast-cell, but that these symptoms are due to the presence of definite chemical compounds, the result of the life of these microscopic organisms. So it is to the action of these poisonous substances formed during the life of the organism, rather than to that of the organism itself, that the special characteristics of the disease are to be traced; for it has been shown that the disease can be communicated by such poisons in entire absence of living organisms.

If I have thus far dwelt on the progress made in certain branches of pure science it is not because I undervalue the other methods by which the advancement of science is accomplished, viz., that of the application and of the diffusion of a knowledge of nature, but rather because the British Association has always held, and wisely held, that original investigation lies at the root of all application, so that to foster its growth and encourage its development has for more than fifty years been our chief aim and wish.

Had time permitted I should have wished to have illustrated this dependence of industrial success upon original investigation, and to have pointed out the prodigious strides which chemical industry in this country has made during the fifty years of her Majesty's reign. As it is I must be content to remind you how much our modern life, both in its artistic and useful aspects, owes to chemistry, and, therefore, how essential a knowledge of the principles of the science is to all who have the industrial progress of the country at heart.

This leads me to refer to what has been accomplished in this country of ours towards the diffusion of scientific knowledge amongst the people during the Victorian era. It is true that the English people do not possess, as yet, that appreciation of the value of science so characteristic of some other nations. Up to very recent years our educational system, handed down to us from the middle ages, has systematically ignored science, and we are only just beginning, thanks in a great degree to the prevision of the late Prince Consort, to give it a place, and that but an unimportant one, in our primary and secondary schools or in our universities. The country is, however, now awakening to the necessity of placing its house in order in this respect, and is beginning to see that if she is to maintain her commercial and industrial supremacy the education of her people from top to bottom must be carried out on new lines. The question as to how this can be most safely and surely accomplished is one of transcendent national importance, and the statesman who solves this educational problem will earn the gratitude of generations yet to come.

In conclusion, may I be allowed to welcome the unprecedentedly large number of foreign men of science who have on this occasion honoured the British Association by their presence, and to express the hope that this meeting may be the commencement of an international scientific organisation, the only means nowadays existing, to use the words of one of the most distinguished of our guests, of establishing that fraternity among nations from which politics appear to remove us further and further by absorbing human powers and human work, and directing them to purposes of destruction. It would indeed be well if Great Britain, which has hitherto taken the lead in so many things that are great and good, should now direct her attention to the furthering of international organisations of a scientific nature. A more appropriate occasion than the present meeting could perhaps hardly be found for the inauguration of such a movement.

But whether this hope be realised or not, we all unite in that one great object, the search after truth for its own sake, and we all, therefore, may join in re-echoing the words of Lessing: 'The worth of man lies not in the truth which he possesses, or believes that he possesses, but in the honest endeavour which he puts forth to secure that truth; for not by the possession of truth, but by the search after it are the faculties of man enlarged, and in this alone consists his ever-growing



*Selfishness*

perfection. Possession fosters content, indolence, and pride. If God should hold in His right hand all truth, and in His left hand the ever-active desire to seek truth, though with the condition of perpetual error, I would humbly ask for the contents of the left hand, saying, "Father, give me this; pure truth is only for Thee."

*it bears its own retribution both  
a miserable god to do this*





MANCHESTER, 1887.

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ADDRESS  
TO THE  
CHEMICAL SECTION  
OF THE  
BRITISH ASSOCIATION.

BY

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.,

PRESIDENT OF THE SECTION.

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LADIES AND GENTLEMEN,—It is, I can assure you, with a feeling of extreme diffidence that I take the chair to-day as President of the Chemical Section at this meeting of the British Association. When I look round me and see the many distinguished men who are prepared to take part in our proceedings I cannot but very strongly feel that the Council's choice might have fallen on a worthier representative of chemical science than myself. Having in the course of my career bestowed more time and attention to technical matters than to purely scientific subjects, and having moreover arrived at a time of life when active participation in work of any kind must necessarily be drawing to a close, you must not expect from me the accurate knowledge of the present state of chemical science and the questions that are at this moment presenting themselves for solution such as would naturally be required from anyone occupying the post which I have on this occasion the honour to hold. The marvellously rapid progress of chemistry during the last twenty years has made it difficult for the most industrious cultivator of the science to keep abreast of the knowledge of the day, and for a *dilettante* like myself one may say it is next to impossible. I confess myself painfully conscious of my defects in this respect, and I shall therefore have to claim the indulgence of the Section should questions arise on which I am unable to speak with authority, or even to discuss with advantage.

Considering, however, how efficiently I am supported by the gentlemen with whom I have the honour to be associated, and to whom I am sure in any case of difficulty I may appeal for assistance, I trust to be able to perform the duties of my office without discredit. I will not, however, trouble you with merely personal questions, which are always more or less tedious, but proceed with the few remarks which I wish to make, and which, if not new or instructive, may perhaps serve to entertain you during the time usually devoted to addresses of this kind.

I think you will hardly expect me, even were I fully competent to do so, to review the progress of chemistry during the last half-century, for the time at my disposal would be too short and the result at my hands, I fear, unsatisfactory. I shall prefer to call attention in a few words to the chemistry of other days as I knew it and the chemistry of the present time as known to us all, and to point out what I consider to be the chief characteristics of each. I shall then, with your permission, point out a few of the directions in which, in my opinion, the chemistry of the future will probably be developed, and in this undertaking I shall perhaps

be more successful than in the other; for to discuss the history of science requires exact knowledge; but in speculating on its future the imagination comes into play, and to imagine is easier than to describe.

When I first entered on my studies, exactly fifty years ago, chemistry could hardly be called a science—it was rather a collection of isolated facts unconnected by any consistent theory covering the whole field. Most of the important elements were known, but of the exact proportions in which they combine together we were ignorant. The law of definite proportion had been generally accepted, but so imperfect were the data then at our disposal that we may say the law was rather taken for granted than proved. The atomic theory of Dalton as explaining this law had also been adopted by chemists; but it is not unlikely that this theory, then in its infancy, might by the vigorous onslaught of a man of Berthollet's acumen have been upset, and we should then have been left entirely without a guide through the bewildering labyrinth of facts. Of any connection between chemistry and physics there was in those days no question; of any but the most superficial notions regarding the effects of heat, light, and electricity on chemical substances we had no conception. The idea that chemistry could have any bearing on or connection with physiology or pathology would have been ridiculed as absurd. I cannot think of the then state of organic chemistry without feeling amused. The state of this branch of chemistry could hardly perhaps be called chaotic or rudimentary, for, after all, what had been done had been well done and neatly done, but the assemblage of facts of which it consisted was devoid of systematic arrangement; it resembled a cabinet of curiosities, the components of which stand in no recognisable relation to one another, or a miscellaneous collection of books placed in an orderly manner on shelves, but without any attempt at classification. As to the genesis of organic compounds, what would now be called absurd notions prevailed. I distinctly remember eminent chemists maintaining that no strictly speaking organic body, even of the simplest constitution, could possibly be formed without the intervention of the so-called vital force. The fact, then recently discovered by Wöhler, of the artificial formation of urea from inorganic substances, was considered as something almost miraculous—*i.e.* as a phenomenon the like of which would never again recur. Without, however, entering into further details, I think I may, without fear of contradiction, assert that the main distinction between the chemistry of fifty years ago and the chemistry of the present day consists in this, that, whereas formerly the science dealt chiefly with qualitative reactions, it now occupies itself principally with quantitative determinations. To have established the fact that every chemical phenomenon may be represented in figures, denoting either number, measure, or weight, such figures, when once accurately determined, remaining constant and unchanged through all time—this seems to me the crowning glory of modern chemistry. It is the firm establishment of this principle that has transformed the face of chemistry and has changed it from a mere descriptive into an exact science.

In justice to our predecessors it should, however, be remembered that this principle, though more fully developed in our own day, was not for the first time set up in quite recent times. The labours of Dalton, conducted on quantitative lines, were performed in this city of Manchester in the early part of this century. At the same time Berzelius was engaged in analysing the most important inorganic compounds and establishing the fact, not previously recognised, strange as it may now appear, that every well-defined substance has a definite chemical composition. But going still further back, we come to the alchemists. Now alchemy, if it has any logical basis at all, is founded on quantitative notions as regards matter. All metals, the alchemists said, consist of sulphur, salt, and mercury (these terms signifying not so much elements in the modern sense as qualities) in various proportions; hence their convertibility. Take copper, remove from it a certain proportion of its sulphurous constituent, and add more of the mercurial, and you have silver; repeat the process with silver, and gold results. At the time of which I speak, though much important analytical work had been done by Berzelius, Rose, and others in inorganic chemistry, though the veteran Chevreul had led the way in placing organic chemistry on a quantitative basis, and the composition of the most



important organic compounds—thanks to the labours of Liebig and his method of organic analysis—had been ascertained, still quantitative determinations were not considered of such paramount importance as at present. In fact, scientific thought did not run in that direction, but satisfied itself, for the most part, with the study of qualitative reactions. It was still possible to see memoirs by eminent chemists containing not a single quantitative determination. Strange as it may seem, two able chemists, Boettger and Schoenbein, were living until quite recently who worked and obtained valuable results without resorting to the balance, the instrument which of all others seems the most indispensable to the chemist of to-day. The balance was indeed universally employed in my younger days, but no other instrument, properly so called, was ever seen in the laboratory. The spectroscope was not yet invented, the polariscope had not come into use; volumetric analysis was still in its infancy. Even the thermometer was but seldom used. What a different picture does the laboratory of the present day present, with its instruments of precision and its various appliances for effecting quantitative determinations of all kinds!

Whether the universal prevalence of and exclusive attention to quantitative methods in chemistry has been an unmixed good may be doubted. Who has not run with a weary eye over the long array of figures, the never-ending tables of which some modern memoirs seem to consist, and not longed for some mere description—were it only regarding trivial matters—to relieve the monotony and fix the subject treated of on the memory? That quantitative determinations given in quite precise terms may occasionally be entirely futile may be seen on referring to the history of alchemy. One of the later alchemists professes to have converted 5,400 parts by weight of copper into 6,552 parts of silver by the action of 1 part of a metal-improving substance (philosopher's stone)<sup>1</sup>. Here we see the quantitative method applied to a purely chimerical process, elaborated from the depths of the experimenter's inner consciousness, and of no value whatever. Much of what is at the present day carefully worked out and presented to the world in numerical form may, like this statement of the alchemist, pass away and be forgotten. This may possibly be the case with the numerous carefully made analyses of water which we now meet with, and which we would gladly exchange for a few decided qualitative tests of its hygienic properties. In the case of air and water it is not the minuteness of the noxious matter which causes doubts to arise, but the absence of any decided and undoubted chemical characteristics of the impurities present. It is probable that a refined sense of taste, uncorrupted by the luxurious indulgences which civilisation has introduced, would be able to detect differences in drinking water which might escape the attention of the most consummate analyst.

Whatever objections may, however, be entertained to the application of quantitative methods in natural science, to the exclusion of others, it is certain that important results have flowed from their adoption, inasmuch that we seem to have arrived at the conclusion that the expression of quantitative results is the be-all and end-all of science; that all differences are merely quantitative; that there is no such thing as mere quality. The whole philosophy of our age is expressed in this one proposition: All differences within the sphere of our experience are quantitative. It is the basis of Darwinism, if I am not mistaken, and underlies many of our political and social theories. Of course it is a mere assumption if stated generally, for the phenomena that admit of purely quantitative expression are few in number compared with those that do not; but then it is surmised, and with some degree of probability, that the vast region outside the quantitative sphere will in time come to be included within it. The past history of science seems to render this likely in the future. The science of chemistry has so far, however, presented an insuperable barrier to the general adoption of this view, and will continue to do so as long as the so-called elements remain what we now admit them to be—indestructible, immutable, inconvertible. It is possible to denote all the known properties of gold and silver, their atomic weight, specific gravity, hardness, malleability, action towards heat, light, and electricity in precise numbers with reference in each case to a certain standard, and yet we cannot say that silver minus a little of this,

<sup>1</sup> Kopp, *Die Alchemie*.



plus a little of that, constitutes gold—the two elements are essentially and radically distinct. Unless we admit with the alchemists that by taking away a little of A and adding a little of B we can convert one metal into another, one element into another, the quantitative method must fall short of its complete development in chemistry. Numerous attempts have, therefore, been made to show the theoretical probability, even if it should not be possible to prove it experimentally, of the so-called elements being really compound bodies, or at least of their containing a basic matter common to all. My predecessor in this chair has endeavoured to show in the brilliant address delivered to this Section on the occasion of its last meeting that the barrier hitherto presented to us by the intractability of our present elements may be overcome, and has adduced experimental illustrations in favour of his views of the compound nature of the elements. Mr. Crookes has called to his aid the doctrine of evolution, which has proved so valuable an instrument in the hands of the biologist, maintaining that the elements, like the species of plants and animals, were gradually evolved by some process of condensation from a primordial matter called by him *protyle*, each step in the process being represented by a distinct element. This is doubtless taking very safe ground, for if the process of evolution was the same in the inorganic as it is supposed to have been in the organic world, the process can never be repeated, and we shall, therefore, never be in a position to illustrate it experimentally. I may however have misunderstood what Mr. Crookes meant to convey, and, if so, must apologise for the dulness of my apprehension. Granting, however, the possibility of our resolving our present elements, were it in theory only, into modifications of one basic material out of which they have been evolved, the question would still remain to be answered, What has caused this primordial matter to be split up into groups and forms having distinct and opposite qualities? and when this question is answered, if it can be answered even in a problematical way, then other questions would arise, until by degrees we should arrive at the confines of physical knowledge and find ourselves in the region of metaphysics, where scientific reasoning ceases and thinking for scientific purposes becomes unprofitable. Excursions into this region would indeed be very useful if on returning to physical regions we could every time bring back with us an instrument as potent and far-reaching as the atomic theory has proved to be, a theory which still remains the basis of all our reasoning in chemistry, but then the atomic theory has been quite an exceptional instance. Metaphysical speculation, such as the *Naturphilosophie* of the Germans has dealt in, has, generally speaking, been utterly barren in natural science.

I will not on the present occasion dwell on the vast addition made to the number of useful and beautiful substances by chemists during the last fifty years. Their number is legion, and their mere description fills volumes, whereas half a century ago a dictionary of moderate size would have sufficed for the purpose. Among these newly discovered substances none are more remarkable than the metals rubidium, cesium, thallium, indium, gallium, the existence of which was revealed by the spectroscope, and which, indeed, would have probably remained unknown but for the labours of Bunsen and Kirchhoff in perfecting and applying that instrument.

I must not, however, omit all reference to a department of chemistry which has been, one may almost say, created within the time to which I am referring—I mean that of synthesis. When I began to study chemistry we only heard of analysis; of synthesis, so far at least as regards organic bodies, we only dreamt as a remote and unattainable region. The only instance then known of the synthesis of an organic substance was that of urea by Wöhler. Synthesis was, indeed, supposed to be an essentially vital process effected under the influence of the vital force, and quite outside the sphere of the chemist. Since then what marvels have we not seen? Alizarin and purpurin, the colouring matters of madder, have been prepared artificially by Graebe and Liebermann, indigo by Baeyer, not to mention bodies of simpler constitution obtained by comparatively less complicated processes. We are honoured to-day by the presence of Professor Ladenburg, who has succeeded in artificially preparing coniin, the alkaloid to which hemlock owes its poisonous properties; the first natural alkaloid, indeed, which has been obtained artificially. Looking back



at what has been achieved I think we may entertain the confident anticipation that all the most important organic bodies, acids, alkaloids, and neutral substances will, in course of time, be obtained in a similar manner, though of one thing we may be pretty sure—viz., that we shall never succeed in forming any really organised matter as distinct from organic. The term organic matter is in fact only employed for the sake of convenience, and as an expression handed down to us from former days, since so-called organic compounds are subject to the same laws with regard to composition as the bodies which we name mineral or inorganic, but organised matter such as we find constituting the vessels of plants and animals is a different thing. The protoplasm contained in the vegetable and animal cell is something very distinct from the same matter after the death of the organism, but the difference between living and dead matter is not of a chemical nature. In referring to chemical synthesis I cannot refrain from expressing regret that so little has hitherto been done in the artificial production of minerals with a view to elucidating the processes by which they were formed in nature, but it is possible that more has been done in this direction than I am aware of, since this is a department of chemistry with which I am not familiar. It is certain that inorganic chemistry generally does not now receive the attention which it formerly did. The exclusive devotion to the chemistry of the carbon compounds which we find in most of our laboratories at the present day may however be accounted for when we see the brilliant results to which the study of those compounds has led.

After these few remarks on the development of chemistry during the last fifty years, of which I know a little, it may seem presumptuous on my part in the presence of some of the most eminent chemists of our day, whose opinions must be of infinitely more value than mine, to say anything about the future of our science and the direction it will probably take. Nevertheless, trusting to your kind indulgence, I will venture on some speculations in this direction, which, if they do not instruct the younger members of the Section, may serve to amuse their seniors, and at all events will refer to subjects on which some thought is well bestowed.

As regards the future of chemistry, the question has frequently suggested itself to me as it has doubtless done to others—Will chemical science go on expanding and developing during the next few generations as it has done in the course of the last hundred years? Will discovery follow discovery, and fact be added to fact, until the record occupies not a few volumes only, but a whole library? Will systematic chemistry, *i.e.* the history and description of all possible combinations of the elements, have any limits? I am inclined to answer in the negative. All human institutions pass through the same phases; they have their rise, they culminate, and decay; and I do not see why the science of chemistry should form an exception. Moreover, it is a natural law that whatever develops rapidly also declines rapidly, and the development of systematic chemistry since the commencement of this century has been perfectly unprecedented. I think it probable that in the course of time, at the rate at which we are now progressing, nearly all possible compounds will have been prepared, all the most important chemical facts will have been discovered, and pure chemistry will then be practically exhausted, and will be in the same condition as systematic botany and mineralogy now are. New compounds will now and then be discovered, just as new plants and new minerals now are, but nothing further will be brought to light that will affect the theories at which we shall then have arrived, whatever they may be. All the material with which the science has to deal having then been brought together, what will happen? Will chemical science cease? Will chemists, satisfied with past achievements, cease to work, confining themselves to practical questions and the history of the days gone by? I think not. The science will continue to develop, but it will be in other directions than those previously pursued. The exhaustion of systematic botany has not put an end to botanical science, for vegetable physiology has opened a wide field to the botanist, one that will take a long time to explore thoroughly. To indicate the directions which chemical science will take in its various applications to other departments of knowledge, as, for instance, in connection with the study of the physical properties of matter, or in elucidation of the chemical processes whereby minerals have been formed, or those



through which geological strata have passed in by-gone ages, would not be within my competency, as I should have to touch on subjects with which I am not familiar; but I may be permitted to refer by a few words to a subject, with which, by reading at least, I have become better acquainted, and which seems to me to offer a wide field to the investigator who shall come well provided with physical and chemical knowledge to its cultivation. I allude to the processes whereby the substances constituting the various organs of plants and their contents are formed, and those again to which the decomposition and decay of vegetable matter are due; a subject as to which our knowledge is quite elementary, but which, it seems to me, admits of an extension and development of which we have at present not the least conception.

De Saussure, it is well known, first discovered the fact that plants under the influence of light absorb carbonic acid and give off oxygen, the inference of course being that the carbonic acid and the water present are decomposed, the carbon of the former and the hydrogen of the latter going to form the various organic constituents of the plant, while the oxygen or a part of it is set at liberty and poured into the atmosphere. The facts as they stand are simply these: what the plant requires for its subsistence is carbonic acid, water, nitrogen in some form (presumably that of a nitrate), certain bases—potash, lime, magnesia, iron oxide, and phosphoric acid. Out of these it constructs the whole of its organic frame, its cells and their contents, re-arranging the elements of which its food consists in such a manner as to convert inorganic into organic matter, *i.e.* changing bodies in which the affinities of the atoms are thoroughly satisfied into such as contain them in a state of more or less unstable equilibrium, and therefore liable to alteration when their atoms are allowed to act in accordance with their natural affinities. More than this we do not know; our ignorance of the several steps or stages of the process, if there are any such steps, is complete; all that has been added to the general statement just given is mere speculation. Yet it is impossible to remain satisfied with the present state of our knowledge on the subject. Accordingly numerous attempts have been made to bridge over the chasm which separates the inorganic and organic worlds, not indeed to show that the change does not involve the creation, as was once supposed, of new matter—for this was proved long ago—but to exhibit in its details the hidden mechanism which produces it, but hitherto without success. We know that light is essential to the process of assimilation in plants, since this does not proceed in the dark, but this fact does not help us to an explanation, for light in this case is a mere stimulant, and never produces the same or similar effects outside the vegetable organism. Liebig and others have attempted to show that the process of assimilation in plants commences with the formation of some simply constituted body, such as oxalic or formic acid, with the elimination of oxygen, out of which by condensation and further separation of oxygen more complex bodies, such as sugar, fats, &c., are formed; but there is not the slightest evidence at present in favour of this view. The first product of assimilation that is distinctly recognised is starch, a highly complex organic, we might almost say an organised body, which appears at once with all its characteristic properties, like Minerva springing fully armed from the head of Jove. If we are to adhere to the facts so far observed, we must conclude that the plant does not proceed as we should do in the laboratory, beginning with the more simply constituted compounds and advancing to the more complicated, but that the reverse process is the one actually adopted, the supposed intermediate products being more probably the results of retrogressive metamorphosis. This conclusion is, however, so much opposed to ordinary chemical views that one cannot feel surprised at the constantly repeated attempts to clear up the question. There can be no doubt indeed that much here remains to be done and to be discovered.

Intimately connected with this subject is that of chlorophyll, the green-colouring matter of leaves, which is always found wherever the process of assimilation in plants is going on, and nowhere else, and is therefore doubtless an essential factor in the process. What part it plays in this process is, in my opinion, still unknown. Its action is probably in part chemical, in part physical, and this adds, it may be, to the difficulty of understanding it. It is generally supposed that it is chlorophyll



which by its direct action on the carbonic acid and water with which it comes into contact leads to the formation of organic matter with elimination of oxygen. But this is, I think, a mere assumption—an error due, like many others, to a mistaken use of terms. The chlorophyll of chemists is simply an organic colouring matter, like alizarin or indigo, but being in the vegetable cell intimately associated with other matters, vegetable physiologists have attributed to the action of one, and that the most obvious, constituent what is really due to the complex, perhaps even to some quite other constituent of the complex. It is impossible to conceive that the chlorophyll of chemists can be endowed with the remarkable and exceptional properties attributed to it by physiologists; it is a chemical entity, nothing more. It may indeed be said that chlorophyll only acts as it is stated to do when enclosed within the vegetable cell, but this merely amounts to saying that its action is not purely chemical, but is controlled by the vitality of the cell, which, I suppose, means the action of the protoplasm. If chlorophyll is the agent whereby the decomposition of carbonic acid and water is effected, how, it may be asked, is the agent itself produced? It does not come from without; the plant must be able to form it in the first instance. We are told by vegetable physiologists that the coniferæ when raised in total darkness from seeds produce chlorophyll. In light or in darkness I am convinced it is the same; the plant forms chlorophyll as a means to an end. What the end is we know; it is the assimilation of carbon and hydrogen to form organic matter. How does the chlorophyll assist in attaining this end?

In propounding a new theory in reply to this question I venture to claim your indulgence, such as has been accorded to some of my predecessors and others who at these meetings of the British Association have been permitted to make statements and use arguments of a novel or paradoxical character, which, if they effect nothing else, at least afford a relief to the usual routine of scientific reasoning. My experiments on chlorophyll have led me to infer that the constitution of that body is much less simple than it is generally supposed to be. I do not mean by this that chlorophyll is a mixture in the usual sense; everyone who has paid any attention to the subject knows that ordinary chlorophyll consists of several colouring matters, some of which are yellow, not to mention fatty matters which are unessential. What I mean to say is this, that the pure green substance, the chlorophyll *par excellence*, does not belong to the same class of bodies as alizarin or indigo, but contains three elements, each of which is essential to its constitution, one being a basic nitrogenous colouring matter, the second a metal or a metallic oxide, the third an acid, the three together constituting green chlorophyll. The basic colouring matter is a body of very peculiar properties; it is the phyllocyanin of Fremy: the metal may be iron or zinc, the acid I will suppose to be carbonic acid. Now the plant having formed its colouring matter, the metallic oxide being present in some form or other, and the carbonic acid being supplied by the atmosphere, all the necessary conditions co-exist for the formation of chlorophyll. The compound is an unstable one; it easily parts with its carbonic acid, giving it up to the protoplasm or whatever the agent may be that effects its actual decomposition under the influence of light. The advantage of this arrangement would consist in this, that the carbonic acid would be presented in a more condensed state to the agent which effects its decomposition than if it were merely contained in a watery solution, but more loosely combined, and therefore more easily accessible than if it were united to a strong base such as potash or lime. The carbonic acid having been disposed of, the other two constituents would be in a state to take up fresh quantities of carbonic acid and so on. Chlorophyll would therefore act as a carrier of carbonic acid in the plant, just as hæmoglobin serves to convey oxygen in the animal economy. Numerous objections may of course be raised to the theory of which I give an outline; I only throw it out as a tentative explanation, showing that the function of chlorophyll may be, in part at least, chemical, and that we need not suppose it to be endowed with the marvellous and exceptional powers usually ascribed to it. Other and more probable explanations will doubtless suggest themselves when this difficult subject has been more thoroughly worked out. Eventually, too, it will be found, I imagine, that



physical forces as well as chemical affinities play a part in this as in every other process of the vegetable economy. In the case of chlorophyll there can be no doubt that the green colour and the peculiar behaviour towards light have something to do with its action, but on this point it is not necessary for the chemist to pronounce any opinion. I may take this opportunity of mentioning the important experiments of Sachs and Pringsheim on the optical properties of chlorophyll in their relation to assimilation in plants, as they are probably not so well known to chemists as to botanists.

What I have said may serve to show that the very first steps of the process whereby organic or organised matter is formed in plants are hardly understood. We understand still less the further steps leading to the production of the more complex vegetable bodies—acids, alkaloids, fatty matters. Granted that we were able to trace the formation in the plant of a compound of simple constitution, such as oxalic or formic acid, how far should we still be from understanding the building up of such compounds as starch, albumen, or morphia? The syntheses so successfully and ingeniously carried out in our laboratories do not here assist us in the least. We know the steps by which alizarin is artificially produced from anthracene; but can anyone for an instant suppose that the plant commences in the same way with anthracene, converting this into anthraquinone, and having acted on the latter first with acid, then with alkali, arrives at last at alizarin? Indeed the plant never contains ready-formed alizarin at all. What we observe from the commencement is a glucoside, a compound of alizarin and glucose, which, so far as we see, is not gradually built up, but springs into existence at once. When we think of the complicated process by which indigo is produced in the laboratory with the various substances and appliances required, and then see how in the minutest seed-leaves of a plant like woad a still more complex substance, indican, is found ready-formed, we stand confounded at the simplicity of the apparatus employed by the plant, and are obliged to confess that we have no conception of the means whereby the end is attained. The same difficulties occur in other cases, and it will therefore probably be conceded that the synthetic processes carried on in plants, from the first step to the last, are not in the least understood.

It might be supposed that after all the labour and attention bestowed on the inorganic constituents of plants we should know something of the part played by these constituents in the processes of assimilation and nutrition, but here the obscurity is as great as elsewhere. We know by experiment that certain inorganic matters—potash, lime, magnesia, iron oxide, phosphoric acid—are essential to the growth of plants; but of their mode of action, or of the reason why certain plants require potash salts, others lime, and so on, we know nothing. Phosphoric acid is no doubt an essential constituent of the protoplasm of the plant; but why cellulose, of which the various organs chiefly consist, should require mineral matters, which do not enter into its composition, for its formation and building up is still a mystery.

The department of chemistry which relates to the decomposition of organic and organised matters presents problems almost as difficult of solution as those relating to their formation and building up; that is to say, the phenomena observed do not apparently obey the same laws as those prevailing in the inorganic world. When I began my chemical studies the difference in this respect between mineral and organic compounds was less clearly seen than at present. The conversion of alcohol into acetic acid, the putrefaction of animal and vegetable matter were thought to be simply due to oxidation; they were phenomena, it was supposed, exactly similar to the rusting of iron, the tarnishing of metals, the fading of colours. That a third body was required to initiate and continue the process of decomposition, that organic matter in contact with purified air would remain unchanged for any length of time—was not known nor suspected. I am not quite sure whether spontaneous decomposition—i.e. the splitting up of a complex body without the intervention of an external agent—might not at that time have been considered possible. In order to explain the phenomena of fermentation, the decomposition of sugar into alcohol and carbonic acid, for instance, we had only the theory of contact—devised by Berzelius and Mitscherlich, the latter of whom used to expatiate on the subject at great



length in his lectures. When this ghost of a theory was laid by Liebig, who suggested an intelligible explanation of the phenomena in accordance with the facts then known, it was felt to be quite a relief, as affording a resting-place—if only a temporary one—for the mind. The brilliant researches of Pasteur, which have thrown so much light on the action of the insoluble organised ferments, I need only refer to, as they are so widely known, even outside scientific circles; and since also investigations such as his cannot be discussed without some reference to biological questions, which cannot be entered on now. I will confine myself therefore to a few remarks on the unorganised or soluble ferments, one of which I had occasion to examine when engaged in investigating madder and its colouring matters. These ferments, the type of which is diastase—a substance found accompanying starch in the seeds of plants—are soluble in water, perfectly neutral, devoid of all definite form, and though apparently inert able when acting within the sphere in which Nature has placed them to cause changes and decomposition of the most profound character. Their action excludes everything in the shape of vitality, and yet it is as mysterious and unaccountable as anything that the vitality of the organised ferments is capable of effecting. Indeed, in vegetable, and especially animal, organisms they seem expressly intended for the attainment of certain ends necessary for the well-being, or even the existence, of the organism, insomuch that it has been supposed, with some show of reason, that it is to bodies of this class existing within the cells of organised ferments, but not separable by any means at our disposal, that the changes produced by the latter are really due.

A great deal of attention has been paid to the products and results of fermentation, but very little hitherto to the *modus operandi* of the ferments themselves, and yet this seems to me to offer a wide field for interesting research, especially in the case of those of the soluble class, which are easily prepared, and can be manipulated in the laboratory like any chemical substance without the tedious precautions and preliminary operations necessary in the case of the organised ferments. In what way, it may be asked, do these soluble ferments produce the effects peculiar to them? Is the action essentially chemical, or is it due to physical causes as well? Is the quantity of fermentable matter acted on by a certain quantity of ferment unlimited in amount, or are there limits to that amount somewhere? Does the ferment itself undergo any change during the process of fermentation, or is it the same afterwards as before and capable of acting on fresh quantities of fermentable matter? When a ferment is replaced by a strong mineral acid, the products of decomposition being the same, is the *modus operandi* in both cases alike, or must a different explanation be in each case sought? These questions have never been satisfactorily answered, and await solution. I know of only one attempt to show what actually takes place during a process of fermentation set up by a soluble ferment.

The experiments of Wurtz<sup>1</sup> on papain, the soluble ferment of *Carica papaya*, led to the conclusion that the fibrin on which it is made to act combines in the first instance with the ferment itself, the latter after the hydration of the fibrin is completed being again set at liberty, and then able to act on fresh quantities of fibrin. Thus, according to Wurtz, the action is found to be the same as that of chemical agents, properly so called, such as sulphuric acid, of which minute quantities may exert a hydrating action in consequence of the transitory formation of compounds which are constantly being produced and again decomposed.

There is another question referring to these soluble ferments to which in the present state of our knowledge it is impossible to frame a probable answer, viz., why does it so frequently happen that each ferment exerts a specific action, an action peculiar to itself, this being in fact, in the absence of any marked chemical characters, the only means by which they can be distinguished one from the other? Why does one ferment act on starch only, while the function of another consists in the hydration of fibrin, that of another in the decomposition of a glucoside, and so on? In accordance with the explanation of Wurtz, we should say that a specific ferment is one capable of combining only with the body on which it is

<sup>1</sup> *Comptes Rendus*, 91, 787.



to act, and with no other. I was led to ask this question when engaged in the examination of the colouring matters of *Rubia tinctorum*. The root of this plant, the madder of commerce, contains glucosides, which, though coloured, are quite devoid of tinctorial power. Nature has at the same time placed in the root a peculiar ferment, which, coming into contact with these glucosides at a certain temperature, effects their decomposition, splitting them up into glucose and true colouring matters. Now this ferment is a body *sui generis* and cannot be replaced by any other ferment that I have tried; its action is specific. Why Nature should have deposited this body in the recesses of the plant for the express purpose of acting on certain glucosides and forming colouring matters, the object of which, so far as the economy of the plant is concerned, can only be guessed at, is difficult to understand. One is inclined in such a case to revert to the old-fashioned doctrine that some natural processes were devised for the use and delectation of man. It is quite certain in the case of madder that had it not been for its peculiar ferment erythrozym, the valuable tinctorial properties of the root, which have for centuries been applied in the production of that splendid dye Turkey red, would have remained unknown perhaps to the present day, since the only efficient substitute for the natural ferment is a strong mineral acid, and such acids and their uses were unknown in former days.

I am inclined to think that some of the younger chemists and physiologists of to-day may live to see the time when all the at present mysterious and unaccountable processes going on in the organisms of plants and animals, including those of fermentation, will be found to obey purely physical and chemical laws. To the biologist it may seem derogatory to the dignity of his science to have the principle of vitality, which has so long reigned supreme, dethroned and replaced by hard, unbending law. Such, however, is not the opinion of that distinguished botanist Sachs, who says, referring to this very point:—‘Der Organismus selbst ist nur die aus verschiedenen Theilen bestehende Maschine, die durch weitere Eingriffe äusserer Kräfte in Bewegung gesetzt werden muss: von ihrer Struktur hängt es ab, welchen Effekt diese äusseren Kräfte an ihr bewirken. Es würde einen sehr niedrigen Horizont wissenschaftlicher Bildung verrathen, in diesem Vergleich eine Herabsetzung des Organismus sehen zu wollen, denn in einer Maschine, wenn auch nur von Menschenhänden gemacht, liegt das Resultat tiefsten und sorgfältigsten Nachdenkens und hoher Intelligenz, soweit es ihre Struktur betrifft, und wirksam sind in ihr schliesslich dieselben Naturkräfte, welche in anderer Combination die Lebenskräfte eines Organs darstellen. Die Vergleichung des organischen Lebens mit unorganischen Processen kann nur dann als eine Erniedrigung des ersteren gelten, wenn man so thöricht gewesen ist, die letzteren als etwas Niedriges und Gemeines aufzufassen, während die unbegreifliche Grösse und Durchgeistigung der Natur in beiden Fällen sich gleichartig offenbart.’<sup>1</sup> The time may be far distant when these views of the great botanist shall be universally accepted; but they will, I think, sooner or later prevail.

The little known territory which separates the domains of chemistry and physiology will, in my opinion, offer a wide and interesting field for research, after that of pure chemistry shall have been exhausted or lost its interest. Most important problems connected with life and its relation to the inorganic world there await solution, and I confess that I am inclined to envy the young investigator who, coming provided with an ample store of chemical and physical knowledge, shall apply himself to the solution of these problems. The pleasures derived from the successful pursuit of such studies belong to the highest and purest that we are able to conceive. I can, however, only repeat what has so often been said before, and what the young man of science should not forget, that a life devoted to research only involves no material rewards; it certainly never secures wealth, sometimes not even honour nor fame. Looked on with indifference or even dislike by the State, the Church, and the public at large, all that the man of science can certainly look forward to at the close of his career is the addition at his hands of a few stones to the vast edifice of truth, and the consciousness of having attained a higher stage of intellectual insight.

<sup>1</sup> *Vorlesungen über Pflanzenphysiologie.*



You may probably expect me, before I conclude, to make some reference to technological matters, to the various chemical arts and manufactures for which the Manchester district is noted. At the last meeting of the British Association in Manchester a report on the condition at that time of manufacturing chemistry in the South Lancashire district, by Sir Henry Roscoe, the late Dr. Angus Smith, and myself, was laid before the Chemical Section. A similar report showing the progress made in chemical technology since that time would have been interesting. Great changes have taken place during the period that has elapsed, especially as regards the alkali trade, and quite a new branch of industry has been developed, that of the coal-tar colours. A description of these new features of our chemical industry with statistics of production would therefore have been acceptable. The idea of a report had however to be given up on account of the difficulty of obtaining reliable information as to details, and in these matters it is the details only which are interesting, the general features of the subject being well known. It can hardly be a matter for surprise, I think, that our manufacturers, considering the active competition to which they are exposed, and the disadvantages under which they labour in consequence of the exclusiveness of foreign nations, should be loth to furnish information which would benefit their rivals in trade. Several interesting papers on branches of chemical industry by gentlemen well versed in them will, however, be read before the Section, and these will, to a great extent, make up for the want of a general report. In the Chemical Section of our Jubilee Exhibition, too, you will see a very fine collection of chemical products, more extensive and beautiful, perhaps, than any previously brought together, and these will give you a good idea of our industrial activity. It would have been interesting to witness step by step some of the processes employed in the manufacture of these various products, but this, I am sorry to say, must not be expected generally.

To some it may seem that this Jubilee Exhibition shows the manufacturing industry and prosperity of this district at least at their highest state of development; that they are now at their meridian, and in the future are doomed to decline. If this be so—and there are certainly indications which seem to favour this view—it would be well for those whose visits here are only occasional to take especial note of the present state of things so as to be able to compare their impressions when they next visit us with those now received, since gradual changes in communities, as in individuals, are more patent to casual observers than to those who are always on the watch.

From some points of view the signs of the times are certainly not encouraging. It should not be forgotten that the manufacturing prosperity of this district depends to a great extent on the ample supply of a product which is brought to us at some cost from tropical and semi-tropical countries to be re-exported in the shape of manufactured goods. A political convulsion abroad, and this, unfortunately, is a casualty that may at any time be expected, or even the determination on the part of other nations to starve us out, however short-sighted such a determination might be, might cut off our supplies and disable us permanently as we were partially disabled twenty-five years ago. If to this be added the fact that foreign nations are becoming increasingly hostile and exclusive commercially, we cannot feel surprise at the dismal forebodings entertained and the confident predictions of decline uttered by some who claim to know all the facts. I ought to apologise for alluding to so gloomy a subject on the occasion of this to a great extent festive gathering, but then men of science like to look at a question not only from a hopeful but from every point of view. Fortunately on this question they are not called upon to pronounce any opinion one way or the other.

Should this be the last time that Manchester shall entertain the British Association in the day of its prosperity, I can only say with the German poet—

Schliesst den Kreis und leert die Flaschen  
Diese Sommernächte feierend,  
Schlimme Zeiten werden kommen,  
Die wir Auch sodann ertragen.

Whether in prosperity or adversity I feel sure that this city will always endeavour to entertain its visitors to the best of its ability. On the present occasion I may with confidence on the part of the chemical world of Manchester offer to the many friends from near and far who honour us with their presence at this meeting a most hearty welcome.



MANCHESTER, 1887.

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ADDRESS  
TO THE  
GEOLOGICAL SECTION  
OF THE  
BRITISH ASSOCIATION,  
BY  
HENRY WOODWARD, LL.D., F.R.S., V.P.G.S.,  
PRESIDENT OF THE SECTION.

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SINCE I received the friendly intimation from Professor Bonney, your distinguished and able President of last year, that the Council of this Association had done me the honour to select me to occupy the presidential chair of this Section which he had vacated, I have been greatly exercised as to what subject to choose for the brief address with which it has now become customary to open the Session. Not that there is any lack of materials ready to hand for the purpose—on the contrary, the subjects embraced by geology are now so varied and extensive that the effort to focus them in a single mind is ever becoming a more difficult task to accomplish, and demands the literary skill of a Lyell or a Geikie to marshal and arrange them from year to year in a manner suitable for presentation to you at our annual gathering.

Foremost in interest must necessarily be that which relates to our Home Affairs, and in this I have been most kindly favoured by Dr. A. Geikie, the Director-General of the Geological Survey of Great Britain, who sends me a brief notice of the progress of the Survey for 1886, taken partly from his Annual Report as Director-General and partly from information supplied by the office through the kindness of Mr. William Topley, our Recording Secretary. The following is the statement which I have received:—

The survey of the solid geology of England and Wales was completed at the end of 1883, and the field-staff has since been occupied in surveying the drift-deposits, making at the same time such revisions of the ordinary (solid) geology as may be necessary. In the north and east of England the drift and solid have been surveyed at the same time. The areas examined in the earlier days of the survey, in the south, centre, and west of England, and in Wales, were done for the solid rocks only.

In order to meet the great need for a general map of England and Wales on a moderate scale, one is being engraved by the Survey on the scale of 4 miles to 1 inch (1 : 253440), and will be issued in fifteen sheets.

A few of the survey memoirs relate to large areas, and give complete descriptions of the formations therein exposed, but most of the memoirs are explanations of special sheets of the map. A series of monographs is now in preparation giving full descriptions of special formations. Mr. Whitaker has charge of that on the Lower Tertiaries; Mr. H. B. Woodward and Mr. C. Fox-Strangways are preparing the Jurassic memoir, the former taking the rocks south of the Humber, and the

latter those of Yorkshire; Mr. Jukes-Browne is writing the Cretaceous monograph; and Mr. Clement Reid that on the Pliocene Beds.

In Scotland some advance has been made in mapping the important and complicated area of the north-west Highlands. The surveyors there were chiefly engaged between Loch Stack and Ullapool, subsequently completing the area about Durness and Eriboll. The other parts of Scotland now being surveyed are the north-eastern and the western side of the Grampians, all south of the latter having been already completed.

Ireland is entirely surveyed with the exception of a small area in Donegal, which will probably be completed this year. This district is of interest from its resemblance to the north-west Highlands, and from the problems which it presents as to the origin of the crystalline schists. The recent discovery of organic remains amongst the Donegal schists adds additional interest to this inquiry.

The publications of the Survey during the past year are as follows:—England and Wales, six sheets of the map, two sheets of horizontal sections, three of vertical sections, and six memoirs; Scotland, three maps and one memoir; Ireland, two maps and six memoirs.

The next matter which has arisen since our last meeting relates to our Colonies, and comes to us in the shape of a message from the retiring President of the Association, Sir William Dawson, who has embodied his ideas in a letter to the President of the Royal Society (Professor Stokes), copies of which have been sent also to all the learned Societies. To the former I am indebted for a copy, accompanied by a favourable report thereon from the Royal Society of Canada.

As the object of this communication is one in which I am sure we, as Englishmen, must all feel a hearty sympathy, appealing as it does to our patriotism in its widest sense, as well as to our devotion for and interest in the science of geology, I feel I shall not need to apologise for introducing it to your notice here.

We are invited by it to enrol ourselves, as geologists, in a Federal Union, composed of all our brethren at home, in our Colonies, and in all the dependencies of the British Crown. Nor are we to stop here, for when this has been satisfactorily accomplished it is suggested that we should invite our English-speaking cousins of the great United States, with whom we are already in such close alliance upon so many objects of common scientific interest, to join our Geological Confederation, and, having thus obtained an overwhelming majority, we are to proceed—without armies or vessels of war—to extend our peaceful conquest over every country on the habitable globe, urging and persuading those countries who have not established geological surveys to do so forthwith, and inviting those who have surveys of their own to join our British Association Geological Union. And when all has been accomplished in this direction our exertions as a confederacy may well be extended to secure the mapping of all those outlying regions of the earth's surface at present imperfectly known or still geologically unexplored.

Suggestions such as these could hardly come at a more fitting and appropriate moment, for are we not now on the eve of the completion of the geological surveys of the British Islands? if such a task can ever be said to be completed which has occupied the attentive study of so many able geologists during the last eighty years or more, and from the very nature of the case must always require additional research and revision.

India, Africa, and our Colonies may all hope for future assistance from the many geological students now being trained in our schools and colleges; who may not be required in the near future for home surveys, and must needs go further afield to win their title of admission to the ancient and honourable order of 'Knights of the Hammer.'

This idea of scientific federation was referred to by Professor Huxley in his Presidential Address to the Royal Society in 1885, and subsequently by the present President (Professor G. G. Stokes) in November last.

If we could devise a scheme by which we might, from time to time, recognise in a suitable manner—whether by corresponding membership, or honorary fellowship, or by medals and awards—as Professor Huxley has suggested, the good scientific



work being done by members of the many societies in our distant colonies of Canada, South Africa, Australia, New Zealand, and elsewhere, that would indeed be a step in the right direction, and would doubtless prove most helpful and encouraging to all our fellow-geologists abroad.

The Geological Society of London, no doubt, to some extent covers this ground; but it should be noticed that in the view of this Society our Colonies and other dependencies are not, and I think rightly, recognised as foreigners, that designation being employed for those who are not in any sense subjects of the Queen.

As a consequence, the geologists of our Colonies are not looked upon as eligible for honorary connection with the Geological Society, and though in the distribution of the medals and awards their work is no doubt noticed, yet that is now so important and extensive that it might be desirable to secure for it a more specific and extensive recognition than has hitherto perhaps been possible.

Might we not also through the home influence we could bring to bear by means of this great section of the British Association succeed in inducing our practical colonial governments to see the enormous commercial as well as scientific gain that must eventually accrue to themselves if they would, with wise liberality, continue to completion their much-needed geological surveys, instead of (as has too often happened) abandoning the work before its end has been attained, or making its maintenance from year to year contingent on the chance discovery of gold, or the successful boring for coal or water—results not always to be attained within twelve months by a geologist in a new country, however good he may be, unless he have a fairy godmother or a divining-rod at his command?

If by means of our confederation such useful and helpful works can be inaugurated, we shall have fulfilled an object well worthy the initiation of Sir William Dawson, and of all those whose names may be connected with so laudable an undertaking.

Nor need such a development of the work of this Section interfere in any way with the labours of the 'International Geological Congress,' which occupies a distinct field of its own; for whatever we might accomplish in carrying out the suggestions put forward by Sir William Dawson would really be in effect to second and support—not to hinder—the work of that most useful body of geologists.

Our next topic relates to Foreign Affairs.

The International Geological Congress, which met in Bologna in 1881, and in Berlin in 1885, will hold its next meeting in London in 1888. This year the Committee of the Congress on Geological Nomenclature will meet during the Association week at Manchester. Professor Capellini, of Bologna, is the President of this Committee, and Professor Dewalque, of Liège, is the Secretary. Its object is to discuss various questions respecting the classification and nomenclature of European rocks, and to report thereon to the Congress in London.

It is quite certain that a large number of Continental and American geologists will be present in London next year, and it rests with English geologists to determine whether the meeting shall be as successful as those which have preceded it. The Berlin Congress left the arrangement in the hands of a small committee of English members (Messrs. Blanford, Geikie, Hughes, and Topley), and advantage will probably be taken of the presence of so many geologists in Manchester to further the organisation of the English meeting.

The occasion of the Congress visiting London next year should also be a sufficient reason to enlist new members here, and it is to be hoped that a very cordial reception will be accorded to all those who come from abroad to attend the meeting. It ought to be a great success, and deserves our warmest sympathies and co-operation.

Geology seems, at present, to be passing through what may not inaptly be termed a transitional or metamorphic period in its history, when old-established ideas are rapidly melting away, and under fresh influences are crystallising out into quite other forms.

'New lights for old' is the popular cry both in science and politics, and, like the Athenians, nothing delights us more than to hear tell of some new thing.

If the proposition lately made by Professor Judd, the President of the Geolo-

gical Society in London, in his recently delivered Anniversary Address, holds good, that mineralogy is the father of geology, it seems not improbable that, like Saturn's offspring, our science is in danger of being devoured by its reputed parent; for certainly mineralogy, in the form of petrology, has of late years most largely occupied the geological field, whilst palæontology, once the favourite child of geology, is in its turn threatened with imminent extinction, as a separate study, by biology, which, without any substantial gain, now replaces, *in name only*, the hitherto better known sciences of botany and zoology.

Indeed, could the views so eloquently put forward by Professor Judd be maintained, mineralogy itself would have to be added to the list of sciences included under biology. But notwithstanding the well-known aphorism of Linnæus—

*Lapides crescunt, Vegetabilia crescunt et vivunt ;  
Animalia crescunt vivunt et sentiunt—*

the growth of the first is of a totally different nature from that which takes place in the last.

Minerals, or more properly crystals, increase or grow in size by additions to their *external surfaces* of molecules of matter identical with themselves. They are therefore as a rule homogeneous throughout, almost rigid, and remain under ordinary circumstances unchanged irrespective of time.

Plants and animals, on the contrary, increase by intussusception, or the taking of matter within their tissues. Their bodies are *not homogeneous*, and they exhibit all the various phenomena of growth and decay.

We stand, then, still like 'watchers on the threshold,' not yet admitted beyond the veil. We are not prepared to include minerals in the study of living beings, nor are we, I submit, any nearer the solution of the problem, What is life? whether we call it '*vitality*' or '*vital force*;' nor can we produce it like 'electricity' or 'electrical force,' by the aid of mechanics. That it has existed ever since our planet became habitable by living organisms is beyond doubt; and since life first dawned it seems equally certain that this '*vital force*' was never at any time extinguished, but, like the sacred flame of Iran, its light has always gladdened our earth with its presence.

I have already referred to the vastness and diversity of the domain which geology claims as her own; indeed, we might, if so disposed, pursue our subject in its *cosmical aspect*, and, inviting the astronomer and the physicist to our aid, proceed to consider the evolution of our earth and its subsequent history as a part of the solar system.

Or, taking up *geognosy*, we might inquire into the materials of the earth's substance and the chief rocks and minerals of which its crust is built up.

Should *dynamics* charm us, then we may study the various agencies by which rocks have been formed and altered, and the frequent changes in relation to sea and land which the terrestrial surface has undergone in former times.

Does *rock-architecture* attract us? It is ours to inquire how the various materials of the earth came to be arranged as we find them—whether wrought by living agents, or ejected by volcanic forces, or laid down quietly by water.

Or is *chronology* the object of our study? Then our task will be to investigate the well-marked succession of the stratified rocks and the sequence of events which they record.

Again, we might prefer the *physiographical aspect of geology*, embracing the history of the features of the earth and the causes which have brought about its varied conditions of continent and ocean, of mountain and valley, hill and plain, making up that grand diversity of surface which constitutes its scenery.

Yet more, it is within our domain as geologists to investigate *the past life of the globe* through all its successive changes and to trace it from its earliest dawn in Præ-Cambrian times down to its grand development at the present day.

One result of the very vastness of this kingdom is that there is a tendency amongst its subjects to form into separate constituencies, and these in an incredibly short time evolve languages of their own, so that, unless this fissiparity can be successfully arrested, we shall speedily repeat the story of 'the confusion of



tongues,' and our geological tower, which once promised by our combined labours to reach grandly heavenwards, may soon cease from building altogether.

This incoherence in our body politic may, I think, be traced to that great development by the microscope in mineralogical geology and petrology, which has no doubt been necessitated by the investigation of those remote Præ-Cambrian or Archæan rock-masses in the north-west Highlands, Shropshire, the Malverns, South Wales, Cornwall, and the west of Ireland, whose fossils, if they ever existed, have been entirely obliterated<sup>1</sup> by the changes which their matrix has undergone, and whose very stratification has been lost by metamorphic action. In such investigations some of our ablest geologists have now been for long occupied with the best possible results, and Bonney, Callaway, Cole, Davies, Geikie, Hicks, Hull, Judd, Lapworth, Peach, Sorby, Teall, and many others have been labouring most zealously on these most ancient sediments, barren though they be, of life forms and often destitute of bedding.

It is refreshing, however, to find Professor Judd at times abandoning volcanoes and turning his attention most successfully to lizard-hunting with Professor Huxley in the Elgin sandstones or studying the micro-organisms in the cores from the Richmond boring or the valley of the Nile; to see Dr. Hicks leaving his patron St. David far behind and digging for bones in the præ-Glacial caves at St. Asaph. Professor Lapworth, too, we see avoiding Cape Wrath and discoursing on the beauties of Canadian Graptolites and the Cambrian rocks at Nuneaton.

Thus there is still a bond of union connecting stratigraphical geology and palæontology and a common ground of interest whereon all geologists may meet. It should then be our endeavour not to dissociate ourselves or our interest from any subject of geological inquiry, but to maintain the union between all branches of our science and with all workers in whatever field they may labour, adopting for our motto the ancient maxim, '*Vis unita fortior est.*'

Especially should we adhere to the study of palæontology, seeing that it is indissolubly connected with one of the earliest chapters in the history of our science. Indeed, through the evidence afforded by organic remains, William Smith (better known by the title given to him by Professor Sedgwick, 'the father of English geology'), was led to those remarkable generalisations as to the identification of strata by means of their contained fossils, which have exercised so great an influence over our own science during the past ninety years, and is still the guiding principle on which our classification of the sedimentary rocks is based. What Wollaston has done for mineralogy and crystallography, William Smith initiated for stratigraphical geology; and we cannot overlook our obligation to Smith whilst we reverence the work of his distinguished contemporary, Wollaston.

Palæontology, or the study of ancient life forms, stands somewhat in relation to geology as the science of archæology does to history, or as zoology and botany to physical geography. But, whereas the investigator of recent living forms deals with entire organisms and can study both their morphological and their physiological history as well as their geographical range, the palæontologist has too often to deal with imperfect remains, many of which have no exact modern representative, and has, in consequence, to look for and seize upon minute characters for his guidance, which the worker on recent forms would probably neglect as too trivial for even specific diagnosis.

The palæontologist, if he would succeed, must in fact be a trained zoologist or botanist, as the case may be, and an accomplished geologist also; such combination of qualities like those possessed by the earlier race of 'naturalists' are less frequently to be met with at the present day. They represent amongst us the same class of men as the 'general practitioner' does in medicine; they are the all-round good scientific men, but not 'specialists.'

Biology, or the study of living things, has now become so vast a field that everyone is compelled to take up some special subject, and in striving to master it he makes his reputation as an authority on this or that group of organisms.

<sup>1</sup> Traces of fossils are said to have been met with in Donegal, and I have just received evidence of Trilobites in the Upper Green Llanberis slates at Penrhyn, hitherto considered unfossiliferous!

There is much to be said in favour of such a method of working, but I hold that everyone who so elects to spend his life must first of all pass through a thorough grounding in general biology, and should on no account take up special work until he has mastered thoroughly the general principles of scientific classification and the various types of organised beings, otherwise he will be for ever viewing all nature with distorted vision, seeing in fact 'men as trees walking.' If as a student he shall have been nurtured wholly on the anatomy of the *sole*, all objects will be viewed from the standpoint of that one-sided fish. If the cockroach has engrossed his youthful studies, all nature will swarm with *Periplaneta orientalis*.

We have to guard against the starting of *student-specialists*. They must begin by being 'general practitioners' if they are ever to do any good in the world of science, and after serving their time in a museum or elsewhere then by all means let each follow his own 'bent' and devote himself to some particular group, as did Davidson to the *Brachiopoda*, to the exclusion of all else.

It is the absence of 'all-roundness' which has retarded more than any other thing the constant interchange of ideas between zoologists, botanists, and palaeontologists, without which science languishes. Biologists as a body do not care to look at or study fossils; they see neither form nor beauty in the petrified fragments of a plant or animal such as would induce them to study these more closely, and they turn to the exquisitely perfect specimens of recent objects in their cabinets with a sigh of relief. But *Nemesis* is at hand, created by our modern system of extreme biological training. The student of to-day is averse to the systematic work of both zoology and palaeontology in our museums, and technically inclined craves for nothing so much as to be allowed to imbed some interesting embryo in paraffin and cut it into 10,000 slices.

As a consequence our museums will suffer unless we can revive amongst our students a taste for and a love of general natural history; such, we mean, as the *taste for nature* which excited the enthusiasm of Charles Kingsley and stimulated the zeal of Charles Darwin. We cannot all sail round the world as did Banks and Solander, Darwin, Huxley, Hooker, Wyville Thomson, Moseley, and so many other naturalists, though the mere act of travelling has now become so ridiculously easy that our own Association awoke one morning in Montreal, and may for aught we know find itself some day in Sydney or Melbourne! But we can fully appreciate Nature in a dredging expedition or feel her influence on a moor or mountain, in a quarry or down a mine.

What we want for our students in these high-pressure days are less frequent attendance in the examination room and a more frequent examination of Nature in the field. Our professors must take their men more often afield, and show them how to collect specimens and familiarise them with the aspects of natural objects as seen *without microscopes*, and they will return to their studies with far better and keener eyesight after their own *macroscopic* vision has been enlarged by contact with Nature.

Whoever then takes up the study of fossils must also be well acquainted with the structure of living animals and plants; he may also be expected to go on adding to his store of biological knowledge—but as some division of labour is absolutely essential, the man who pursues palaeontological research must be prepared to concentrate all his energies to the elucidation of these extinct organisms, studying, but not occupying himself in describing, recent forms.

In order, however, to work satisfactorily at any particular group of extinct organisms, his eyes and his understanding must go through a long and careful training before he will be able to interpret correctly the appearances presented by the specimen before him, and to avoid the fallacies by which he is liable to be misled arising out of the necessarily imperfect materials and their different modes of preservation in the matrix.

He must learn to distinguish between a suture and a fracture, and to know when a specimen has been distorted by cleavage or other mechanical cause, or altered by mere difference of mineralisation. Such deceptive appearances have too often led to the multiplication of species, and even the creation of spurious genera.



Thus occupied in the investigation of ancient life forms, he will in truth be only writing the first chapters on the botany or the zoology of the earth, and, whilst his carefully obtained results are of the greatest importance to the speculations and conclusions of the geologist, they are equally essential to and a part of biological science.

My friend Dr. Traquair has recently thus expressed, in relation to his own subject, what I have attempted to make more general:—‘The man who satisfactorily investigates the structure or determines the systematic position of a fish or reptile *preserved in stone* is as much a zoologist as he who describes a similar creature *preserved in spirits*, though with this difference, that the former task is in some points rather the more difficult, seeing that we have *only the hard parts* to go upon, and these generally in a crushed, fragmentary, or scattered condition. And,’ he adds, ‘without a genuine interest in, as well as a thorough knowledge of, recent biology no one can hope to produce work of any value in palæontology.’

Of course the value of all palæontological work, as of all zoological or botanical work, must depend entirely upon the care and exactness with which the work is performed.

Time, the great assessor of all human labours, will sit in judgment upon them and pronounce by their durability or instability the comparative value of each.

It appears to me that to the careful palæontological worker, as to the careful archæologist, the greatest merit is due if he succeed correctly in deciphering the too often fragmentary and blurred remains of a bygone age, and giving us in the present an accurate interpretation of a page from the life history of the past.

Then, too, there is the geological aspect of palæontology. And here I may state that one of the charges made by a brother zoologist against us is ‘that we use fossils *merely* as counters by which to record the progress of geological time.’

As well might exception be taken that the milestones along a turnpike road had been used by a traveller to calculate the length of his journey.

But omitting the word *merely* (for fossils have been made to give up many secrets to the investigator besides their age), I gladly accept the charge as conveying a great and important truth.

Do not the historian and the antiquary use the coins and medals dug from the ruins of the dead and long past dynasties of the world as sure guides in the chronology of the human period?

And may not the geologist also use ‘the medals of creation’—as Dr. Mantell aptly called them—coined in no counterfeit mint, as the best and most trustworthy guides to enable him to establish the chronology of the stratified rocks of the earth?

Great then as is the benefit which zoology has derived from palæontology in enabling the zoologist to learn the earliest appearance in time of each group of organisms, and the modifications in structure, so far as we are enabled to ascertain them, which each may have undergone from the ancient to the modern period—it may be doubted whether even this valuable aid equals the service performed to stratigraphical geology by the careful study of organic remains—in enabling us to write the chronology of the rocks over so large a portion of the habitable globe.

Without fossils stratigraphical geology would be as unsatisfactory as it would certainly be uninteresting; with their aid it becomes, both in the field and in the cabinet, one of the most attractive and delightful of studies.

Owing to the very nature of sedimentary deposits, being of necessity either lacustrine, estuarine, or marine in origin, our knowledge of the ancient land surfaces of the globe is necessarily very limited, but we know much concerning its old marine areas. These are the more constant and widespread, and it is mainly upon these deposits, and not so much upon the more limited evidences of ancient land surfaces, that our chronology has been based.

Of the antiquity of cave-folk and their contemporary mammalia we may expect to hear the very latest utterances from Professor Boyd Dawkins and Dr. Hicks. The former is also to be congratulated upon his renewed work on the Mammalia in the Palæontographical Society’s volume for 1886 (just issued). Professor O. C. Marsh has added a further contribution to American palæontology in the shape of a memoir describing and figuring sixteen new species of Mesozoic

mammals from the Upper Jurassic rocks in Wyoming, on the western slope of the Rocky Mountains. Mr. Lydekker has just completed Part V. of his most useful and much-needed Catalogue of the Fossil Mammalia in the British Museum, containing the Sirenia, Cetacea, Edentata, Marsupialia, and Monotremata.

The fossil birds remain to be catalogued. In the Reptilia it is refreshing to see Professor Huxley once more taking up the pen and writing upon *Hyperodapedon* and *Rhynchosaurus* in his old vigorous and earnest style. We can only regret that his health precludes him from continuous labour, to the no small loss of science. Professor Marsh shortly promises us his memoir on the Sauropoda, the plates of which are progressing rapidly to completion.

Our late veteran chief, Sir Richard Owen, although retired from active official duties, contributes a paper on *Galesaurus planiceps*, a Triassic saurian from South Africa, and a further memoir on *Meiolania* from Lord Howe Island.

Professor Seeley and M. Louis Dollo are both occupied with Dinosauria, the former from the Cape (whence he has also detected part of a mammalian skeleton in the Triassic rocks), and the latter is adding to our knowledge of Iguanodon and other forms from the Wealden of Bernissart.

In the Amphibia, Professor Dr. Herman Credner has added a most valuable paper on the development of *Branchiosaurus*, a small Labyrinthodont from the Keuper of Saxony, in which he has been able successfully to trace the development through a long series of individuals of a water-breathing naked larva of the Palæozoic epoch into an air-breathing adult form, clad in a strong coat of mail.

In fossil ichthyology, A. Wettstein has been occupied in the study of the Eocene fishes of the Glarus slates, and in his recent memoir he shows that out of one fish (*Anenchelum*), so constantly distorted by slaty cleavage, Agassiz had made no fewer than six species. This fish is now found to be identical with the living 'scabbard-fish,' *Lepidopus*; and the author reduces the forty-four species of Glarus fishes to twenty-three and adds four new ones. Among the latter is the first fossil *Remora* yet met with, named *Echeneis glaronensis*. Its first dorsal is modified as a sucker, exactly as in the living *Remora*.

Baron Zigno, of Padua, has figured and described the first entire *Myliobatis*, hitherto discovered in the Eocene of Monte Bolca.

M. Louis Dollo records the occurrence of two skeletons of *Carcharodon heterodon* in the Eocene of Boom, Antwerp, one measuring 7 mètres, the other nearly 9 mètres in length. They are now mounted and exhibited in the Brussels Museum.

Mr. J. F. Whiteaves is commencing to publish the detailed descriptions of the Devonian fishes from Scaumenac Bay, Quebec.

Mr. James Wm. Davis, of Halifax, has produced a second monograph for the Royal Dublin Society. The first, which appeared in 1883, was devoted to the teeth and spines of Elasmobranch fishes from the Carboniferous limestone of Great Britain; the present monograph, illustrated by twenty-four plates, is devoted to the description of the fishes of the Cretaceous rocks of the Lebanon, and makes us acquainted with a wonderful series of Selachian fishes, representing nine genera and sixteen species, of which two genera and twelve species are new to science. The Ganoids comprise two species of Pycnodonts and two forms related to *Amia*; there are also a number of Teleostean fishes, amongst which are *Pagellus*, *Beryx*, *Homonotus*, *Platax*, and many other genera. Two species of eel, *Anguilla*, are the first Mesozoic examples recorded. Altogether we have ten genera and sixty-three species of fish recorded as new. The author is to be congratulated upon having contributed to fossil ichthyology one of the most extensive works published in recent years.

Mr. Arthur Smith Woodward (a former student of Owens College, Manchester) has this year also contributed numerous papers on fossil fishes: on *Ptychodus* from the Chalk; *Squaloraja* from the Lias; on the Brazilian genus *Rhacolepis*; on a Maltese *Holocentrum*; 'On some Eocene Siluroid Fishes from Bracklesham'; and 'On the Canal-system in the Shields of Pteraspidean Fishes.'

Mr. E. T. Newton describes a *Semionotus* from the Trias of Warwickshire.

Both Mr. James W. Davis and Dr. R. H. Traquair have given us descriptions of the anatomy of *Chondrosteus acipenseroides* from the Lias of Lyme Regis.



Mr. William Davies describes two species of *Pholidophorus* from the Purbeck beds of Swanage, Dorset.

But the groups which have proved of the greatest service in the chronology of the sedimentary rocks have been the Mollusca, the Brachiopoda, and Crustacea (especially the Trilobita, Phyllopora, and Ostracoda), the Echinodermata, Corals, Graptolites, Sponges, and Foraminifera.

It would be an interminable task merely to record the workers in the various sections of palæontology, but in glancing at these one cannot prevent many illustrious names arising in one's mind—many who have finished their work, and are reckoned among the fathers of the science, but many also who are still our companions, and from whom we may expect further important help before they lay down their hammer, their lens, and their pen.

In the Cephalopoda the task so lately left by our countryman Dr. Wright, after a long life devoted to palæontological science, has been taken up by Mr. S. S. Buckman, who has already presented one fasciculus of a monograph on the Ammonitidæ of the Inferior Oolite.

The Gasteropoda of the Oolites have an able historian in Mr. W. H. Hudleston, whose contributions on this subject enrich the pages and plates of the 'Geological Magazine' and the 'Proceedings of the Geologists' Association'; the Palæozoic forms are in the hands of Dr. Lindström.

The Lamellibranchiata cry for help at present in vain, and we regret more than ever the loss of Stoliczka, who promised such good work had his life been spared.

The Brachiopoda, so long and so well cared for by Dr. Davidson, now also demand a successor to that illustrious name.

The Polyzoa, which suffered so severe a loss in the death of Mr. Busk, have since been well cared for by Mr. Arthur W. Waters and Mr. Vine.

Until quite lately, the oldest fossil insects known were the six fragments of wings of Neuroptera, from the Devonian of New Brunswick, obtained by Mr. C. F. Hartt and described by Mr. S. H. Scudder. More lately the wing of a cockroach has been obtained from rocks of Silurian age in Calvados, France; whilst almost simultaneously fossil scorpions have been met with by Dr. Hunter, of Carlisle, in the Upper Silurian of Lanark, and determined by Mr. B. N. Peach, and from the Upper Silurian of Gotland, described by Dr. Lindström.

These discoveries carry back our records of old land surfaces to a far more remote period than that of the coal-measures, vast as its distance is removed from recent times.

Mr. B. N. Peach is the discoverer of several scorpions, and I have also recently figured and described three new forms of cockroach and several spined myriapods from the Coal-measures. Another cockroach, also new, which has been kindly sent me for study by Mr. Peach, brings to our knowledge a larval stage of *Blatta* from the Scottish Carboniferous.

Dr. McCook has just added a genus of spiders, *Atypus*, to our Eocene beds from the Isle of Wight.

The Crustacea have found in Mr. B. N. Peach and in Professor Rupert Jones able and willing historians. Mr. Peach has taken up the Carboniferous Macruran Decapods, and Professor Rupert Jones the Palæozoic Phyllopora, aided by myself; Professor Jones is attacking the Tertiary and Cretaceous as well as the Palæozoic Ostracoda, so that his hands will be full for many years to come.

The Echinodermata have lost Dr. T. Wright, who for years acted as their monographer in the Palæontographical Society's volumes, but they have secured the services of other accomplished naturalists. Mr. Robert Etheridge, jun., and Dr. P. Herbert Carpenter have produced a grand monograph on the Blastoidea in the British Museum; and no doubt this is but the beginning of good things to come, for although Mr. Etheridge has entered upon a new sphere of work in the Australian Museum, Sydney, Dr. P. Herbert Carpenter hopes to take up the stalked Crinoids before long, and Mr. Percy Sladen, who, with Professor P. Martin Duncan, has already done so much good work amongst the Indian Echinoderms and elsewhere, promises to take the star-fishes in hand for us later on.

The Corals have many friends, chief amongst whom is Professor P. Martin Duncan, and Professor H. A. Nicholson, and various other excellent workers, but they are even a more difficult and a less attractive group than the Echinodermata, and their determination is not so satisfactory owing to their irregular and heteromorphic growth.

The Stromatoporoids have lost an investigator in the field in Arthur Chamberlaine, whose unexpected and early loss we all deplore. But in Professor Nicholson they will find a most careful and painstaking monographer, who has already given us one fine instalment of his work in the Palæontographical volume.

In Professor C. Lapworth we have an exponent of the structures and affinities of the Graptolites as a class and of their stratigraphical position in the rocks unsurpassed by any other worker. With him must be associated the names of Barrande, Carruthers, Hopkinson, Nicholson, and a long list of foreign workers, all of whom, however, look upon Lapworth as the highest authority in this group.

In the Spongiata we are especially indebted to Dr. G. J. Hinde, first for an excellent, well-illustrated quarto catalogue of these organisms in the geological collection of the British Museum, and secondly for the Palæozoic part of a fine monograph of these for the Palæontographical volume just issued.

Not must we omit to recall the names of Professor Zittel, of Dr. Carter, of Professor Sollas, and many other able workers in the fossil sponges.

In the Foraminifera we naturally recall the names of D'Orbigny, D'Archiac, Carpenter, Parker, Brady and Jones, and Sir William Dawson, our illustrious ex-President. Professor Rupert Jones is still at work on this group, and has recently published a paper on *Nummulites elegans* from the Eocene beds of Hampshire and the Isle of Wight.

Of late years fossil Botany, too long neglected, has taken a place of note in all those inquiries concerning the origin of floras, the age of the stratified rocks, the former distribution of land surfaces, and especially in all questions relative to the climate of the globe in past times.

Passing over the earlier period of the present century, when fossil botany was known only by the works of Artis, Witham, Schlotheim, Sternberg, Goeppert, Cotta, Lindley and Hutton, Steinhauer and Adolphe Brongniart, we have to recall the names of other workers who have only passed away in our own time, such as Binney, Bunbury, Corda, Bowerbank, Heer, Unger, Schimper, and Massalongo.

In the period of fifty years, whose completion we have just celebrated, the names of our countrymen Binney, Bowerbank, Williamson, and Hooker stand prominently forward contemporarily with those of Geinitz, Unger, Rossmasler, and Schimper in Germany. In 1845 Dawson and Lesquereux entered the field in America, Hooker in England, and one of the ablest writers on fossil plants, Oswald Heer, entered upon his great work in Switzerland. In 1850 Massalongo in Italy, and von Ettingshausen in Austria, were added to the roll of famous palæobotanists, and in 1853 Newberry joined the American field of research. In 1860 the work so long abandoned by Brongniart, in France, was taken up by de Saporta, and it is no small gratification to have him with us here to-day, and to welcome him amongst our distinguished foreign guests.

About the same time my friend and colleague William Carruthers commenced to write on fossil botany, and brought to bear upon the subject that accurate and careful knowledge of living forms without which such investigations must always prove but futile.

It is extremely difficult to estimate the number of species of fossil plants that had been described up to the year 1837, but it probably fell far short of a thousand. In 1828 less than 500 species were known to Brongniart.

In the first edition of 'Morris' Catalogue,' published in 1843, the number of British fossil plants recorded is 628.

Careful lists were published by Goeppert and by Unger in 1844 and 1845, giving a total of known species from 1600 to 1800.

In 1849 the number had increased, according to Bronn's 'Index Palæontologicus' to over 2,000, and the following year Unger enumerated 2,421 in his 'Genera et species Plantarum,' rather more than 500 of which may have been British. In



1852 Morris (2nd edition) gives the number of species as 740. Since then, chiefly through the labours of Heer, Ettingshausen, Lesquereux, Massalongo, Unger, and de Saporta, this number has been more than quadrupled. Mr. Gardner estimates that at least 9,000 species must have been described. This great increase is chiefly due to the more careful exploration of the Tertiary strata, in which the more highly organised and consequently more differentiated plant forms occur.

The number of plant remains described in Great Britain during the whole 50 years has been extremely small, but much has been accomplished in the study of fossil plants generally, and in this task no one has been more earnest than Professor Williamson, of Owens College, Manchester.

His investigations of the plants of the coal period have been of the most exhaustive nature, and from his researches into their microscopic structures we are almost as well acquainted with the minute tissues of these ancient denizens of the forests of the Carboniferous epoch as we are with those in the parks around Manchester to-day.

Mr. Carruthers' 'Memoirs on the Coniferæ and Cycadeæ, and on the Fruiting Organs of the Lycopodiaceæ' have greatly advanced our knowledge of these interesting types, heretofore but imperfectly known from their fossil remains.

Mr. R. Kidston has devoted himself most earnestly to the investigation of the fossil plants of our British coalfields, and he has determined not to rest satisfied merely to work out the plants obtained by others in our museums, but he has visited all our coalfields and searched the shales on the spot for himself. The results of his collectings may now be seen in the valuable additions made to the coal-measure series of plants in the British Museum (Natural History).

But it is more especially in reference to the Tertiary flora of Britain that progress has been made of late years.

Thanks to the labours of Mr. Starkie Gardner, who has not only obtained abundant materials for an exhaustive monograph with his own hands from Sheppey, Alum Bay, Bournemouth, Reading, Mull, Antrim, and many other localities, but has already favoured us with several memoirs in the Palæontographical Society's annual volumes and elsewhere on the British Eocene flora, we may hope before long to have a more complete history at this period of our islands than we already possess of the flora of the Carboniferous age.

Nor has any research, favoured by the aid of this Association, brought so large a return in beautiful and instructive specimens to our National Museum of Natural History as have the investigations carried out by Mr. J. S. Gardner.

We must not omit to mention Mr. Clement Reid, who has so diligently traced many of the specimens of our existing flora in the Pleistocene strata of the eastern counties.

'Large numbers of ferns and gymnosperms' (says Mr. Gardner) 'have been discovered in Mesozoic rocks, but remains of the interesting monocotyledons which must have accompanied them are provokingly scarce. We know that palms, grasses, &c., appear at certain definite horizons, but we are ignorant regarding their ancestry. We know that temperate floras, largely composed of dicotyledons, flourished as far north as man has been able to penetrate in the Cretaceous and Tertiary periods, but nothing in the least suggesting a transitional form has been found amongst them. Lastly, we have learnt that floras now indigenous to Japan and the Himalayas, to Australia and South America, once inhabited Europe, groups of wholly different plants succeeding and displacing each other in rapid succession on the same spot as to suggest that the normal condition of floras is one of slow but perpetual migration, and that the term "indigenous" has no geological significance.'

In reference to the question of geographical distribution of organised beings in geological time, the conclusion is strongly forced upon us, from a study of fossil remains, that the great zoological provinces into which the earth's surface and the seas of the globe are now subdivided have been brought about by the limitation of species at no more distant date than the Secondary period, and probably even later than this.

That in Palæozoic times there must have been a great uniformity of marine

conditions, and the fauna of each of the primary formations was consequently not only of vast duration but of world-wide extent.

When, as in Carboniferous times, we are enabled to study the contemporary land conditions of the globe, we find they must also have been very uniform, at least so far as the explored parts of this hemisphere are known, both the fauna and flora at this epoch being co-extensive with the northern hemisphere, indeed in all probability far wider, seeing that identical species occur in the Carboniferous series of Australia and North America. Even those well-marked lines which at present follow more or less closely the isotherms of our hemisphere seem not to have exercised the same influence on the fauna and flora as they do at present. Thus in high northern latitudes and within the arctic circle we find abundant evidence of life in Palæozoic, Mesozoic, and even down to Tertiary times, unaffected by latitude; so that we are justified in assuming that a far milder temperature extended to much higher northern regions than that which at present exists on the globe, and consequently that a larger portion of the earth's surface (as well as its seas) was then habitable.

How great, then, is the field of research still open to our investigation, and how far distant must that day be ere the last problem shall have been solved, and the last chapter written, in the ancient life-history of our earth!

We write in sand, our labour grows,  
And with the tide the work o'erflows.

With unskilled hand I have struck here and there only a few chords on the many-toned harmonicon of geology. I fear they may not all have vibrated quite in unison as a perfect composition would; but, however crude the performance has been, I trust that it will not be provocative of discord. If some few ideas suggest themselves as worthy of your acceptance I shall not have spoken altogether idly, nor you have listened so long and so patiently entirely in vain.



MANCHESTER, 1887.

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ADDRESS  
TO THE  
BIOLOGICAL SECTION  
OF THE  
BRITISH ASSOCIATION,

BY

ALFRED NEWTON, M.A., F.R.S., F.L.S., V.P.Z.S., ETC.,

Professor of Zoology and Comparative Anatomy in the University of  
Cambridge,

PRESIDENT OF THE SECTION.

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IN opening the business of this Section I cannot but call to mind the last occasion when the British Association met in the city of Manchester, just six-and-twenty years ago; and, while my memory brings back to me many pleasing recollections of that gathering, I cannot help dwelling upon the extraordinary difference between the state of things that then existed and that which we have before us to-day. The moral of the contrast I shall not seek to enforce. Those, if any there still be, who despair of the future of our Association may reflect upon it at their leisure; while those who believe, as I do, that our Association has no justifiable cause for thinking that its work is accomplished, that it had better settle its worldly affairs, and compose its robes around it in a becoming fashion, before lying down to die, will at once appreciate the difference.

Yet there is one difference between our proceedings to-day and those of more than a quarter of a century since which I, personally, do not appreciate. In that remote and golden age it had not become obligatory on the President of this Section to prepare beforehand an Address to be delivered to a critical, even though kindly, audience. A few words of friendly greeting to old faces, and a hearty welcome to those that were new, with a general statement of the objects of our coming together, comprised all that was expected from the occupant of the chair. Such was the case when my predecessor, who was, I may observe, my excellent friend and colleague, Professor Babington, opened the proceedings of this Section—then called the Section of Zoology and Botany—at Manchester in 1861; and I am sure I have reason to envy his happy lot, for, on refreshing my memory by turning to the Report of that meeting, I find that his introductory ‘Remarks’ occupy a space of less than eight lines of print. In this respect, but in this only, I must confess myself *laudator temporis acti*, and it having now been for so many years the practice of your President to deliver an Address on occasions like the present, I feel that I should be filling my position under false pretences did I not conform to established usage, though I am well aware that what I have to say will, for many reasons, hardly bear comparison with what has been said by many of my distinguished predecessors.

But to continue the contrast of what took place in this Section at our last  
1887. D

meeting in Manchester with what may be expected to happen now, I would remark that the year 1861 was one which, when the history of biology comes to be written, will be found to deserve particular recognition. This is not merely because of the all-important discovery of *Archæopteryx*, for that had not been made known when the Association met, and did not affect our proceedings here. When we met, it was a time, so to speak, of 'slack water'; but slack water is commonly the effect of two contrary streams, and perhaps I ought to state how this came about. All present should be aware that it was before the Linnean Society on the First of July, 1858, that the stupendous announcement was made of a theory which for the first time brought to the notice of biologists a reasonable explanation of the mode by which what had hitherto passed under the name of the Transmutation of Species could be effected. It is notorious that this announcement attracted but little attention at first, and, though it were easy to account for this fact, I see no need to occupy your time by so doing. I would, however, beg your attention to another fact which is by no means notorious. So far as I am aware, the first zoologist publicly to accept and embrace the theory propounded on that memorable evening on behalf of Mr. Darwin and Mr. Wallace was my old friend Canon Tristram, and moreover he did this ere little more than a twelvemonth had expired.<sup>1</sup> To me it will always be a matter of rejoicing that the adoption of this theory was so early accepted, and additional evidence in its favour adduced, by one who has devoted so much time and energy to the particular branch of zoology which has long recommended itself to me; for thereby I hope that the study of ornithology may be said to have been lifted above its fellows. This, however, is a digression, for introducing which I trust I may be pardoned. And now to return to my main business. Late in the autumn of 1859, as you know, Mr. Darwin's essay on the 'Origin of Species' appeared—a mere abstract, as it still remains, of an enormous mass of materials industriously accumulated by him through many long years—a mass out of which, as he himself has modestly said, a competent man might have written "a splendid book"—but a mass with which he, chiefly through ill-health, had been unable to deal properly. Yet I am not sure that we have any reason to lament the result. The handy size of that celebrated little volume gave it a power of penetration and circulation that would not have been possessed by a work of greater bulk, while the studied absence of technicalities and of reference to scientific authorities in the form of foot-notes (which last, I need scarcely point out, would have largely increased its dimensions) brought its closely-reasoned argument within the comprehension of hundreds whom it would have at once repelled had it been made up of learned phraseology.

Much of what followed on the publication of this work will be in the recollection of many of my audience, while the rest must have heard of it from their seniors. The ever-memorable meeting of this Association at Oxford in the summer of 1860 saw the first open conflict between the professors of the new faith and the adherents of the old one. Far be it from me to blame those among the latter who honestly stuck to the creed in which they had educated themselves; but my admiration is for the few dauntless men who, without flinching from the unpopularity of their cause, flung themselves in the way of obloquy, and impetuously assaulted the ancient citadel in which the sanctity of 'Species' was enshrined and worshipped as a palladium. However strongly I myself sympathised with them, I cannot fairly state that the conflict on this occasion was otherwise than a drawn battle; and thus matters stood when in the following year the Association met in this city. That, as I have already said, was a time of 'slack water.' But though the ancient beliefs were not much troubled, it was for the last time that they could be said to prevail; and thus I look upon our meeting in Manchester in 1861 as a crisis in the history of biology. All the same, the ancient beliefs were not allowed to pass wholly unchallenged; and one thing is especially to be marked—they were challenged by one who was no naturalist at all, by one who was a severe thinker no less than an active worker; one who was generally right in his logic, and never wrong in his instinct; one who, though a politician, was invariably an honest man—I mean the late Pro-

<sup>1</sup> *Ibis*. October 1859, pp. 429-433.



fessor Fawcett. On this occasion he brought the clearness of his mental vision to bear upon Mr. Darwin's theory, with the result that Mr. Darwin's method of investigation was shown to be strictly in accordance with the rules of deductive philosophy, and to throw light where all was dark before.

Now the reason why I have especially mentioned this essay of Professor Fawcett's is not merely that the approval of the disputed theory by such a man did not a little contribute to the success which was then impending, but because I have for a long while maintained that, as a matter of fact, what is now known as the Darwinian theory did not, except in one small point, require a naturalist—and much less naturalists of such eminence as Mr. Darwin and Mr. Wallace—to think it out and establish its truth. Pray do not for a moment imagine that I wish to detract from the value of their demonstration of a discovery that is almost unrivalled in its importance when I say that the demonstration might have been perfectly well made by any reflective person who was aided by that small amount of information as to the condition of things around him, which is presumably possessed by everybody of common sense. It might have been perfectly well made by any of the sages of antiquity. It might have been as well made by any reasoning man of modern time, even though he were innocent of the merest rudiments of zoology or botany; and, as is admitted, the discovery was partly and almost unconsciously made by Dr. Wells in 1813, and again by Mr. Patrick Matthew in 1831—neither of whom pretended to any special knowledge of those branches of science. It is equally a fact that anyone who applied the doctrine of Malthus, the political economist, to the animal and vegetable populations of the world, could have seen that what came to be called 'Natural Selection' was the necessary consequence of the principles enunciated by him; and we have Mr. Darwin's acknowledgment that his reading the 'Essay' of Malthus was with him the turning-point which settled his conviction as to the soundness of the crude speculations in which he had been indulging. Moreover, years before Malthus wrote, a great French writer, though no naturalist, had pointed out, in terms that were *mutatis mutandis* repeated as regards plants at a later time by the elder De Candolle, that all animals were perpetually at war; that each, with a few exceptions, was born to devour others; and that the males of the same species carried on an internecine war for the females.<sup>1</sup> The fact of the 'Struggle for Life' being thus recognised all the rest should follow, and really no close acquaintance with natural history was needed to guide an investigator to the end so far reached.

But in order to see the effect of this principle upon organic life the knowledge—the peculiar knowledge—of the naturalist was required. This was the knowledge of those slight variations which are found in all groups of animals and plants—a point on which I need not now dwell, for to my present audience it must be known in thousands of instances. Herein lay the triumph of Mr. Darwin and Mr. Wallace. That triumph, however, was not celebrated at Manchester. The question was of such magnitude as to need another year's incubation, and the crucial struggle came a twelvemonth later, when the Association met at Cambridge. The victory of the new doctrine was then declared in a way that none could doubt. I have no inclination to join in the pursuit of the fugitives.

But in tracing briefly, as I am now doing, the acceptance of the teaching of Mr. Darwin and Mr. Wallace, there is one point on which I should like to dwell for a few moments, because it has, so far as I know, been very much neglected. This is the great service rendered to the new theory by one who was its most determined opponent, by one of whom I wish to speak with the utmost respect, by one who was thoroughly a philosophical naturalist, and yet pushed his philosophy to overstep the verge of—I fear I must say—absurdity. I mean the late Professor Louis Agassiz, whose labours in so many ways deserve far higher praise than it is

<sup>1</sup> Tous les animaux sont perpétuellement en guerre; chaque espèce est née pour en dévorer une autre. Il n'y a pas jusqu'aux moutons et aux colombes qui n'avalent une quantité prodigieuse d'animaux imperceptibles. Les mâles de la même espèce se font la guerre pour les femelles, comme Ménélas et Paris. L'air, la terre et les eaux sont des champs de destruction.—Voltaire, *Questions sur l'Encyclopédie par des Amateurs*, article 'Guerre.'

in my power to bestow. There must be many here present who will recollect the time when the question "What is a 'Species'?" was always coming up to plague the mind of every zoologist and botanist. That question never received a definite answer, and yet every zoologist and botanist of those days felt that an answer ought to be given to it; for without one they knew that they were sailing on an unknown sea, and that theirs was likely to be lost labour. The chief reason why no answer was given lay in the fact that hardly any two zoologists or botanists could agree as to the kind of reply which should be made, for hardly any two of them could agree as to how a 'Species' was constituted. It will be enough for me to say now that Louis Agassiz pinned his faith on every 'Species' being not merely the result of a single direct act of creation, but, when he found that physical barriers interposed (as they often do) between two or more parts of the area which the 'Species' occupied, he did not hesitate to declare that a 'Species' might have been created directly in several places, at sundry times, and even in vast numbers. If the same Species of freshwater Fish, for instance, was found in several rivers which had no intercommunication, it had been, he asserted, separately created in each. Before his time people had been content to talk of each Species having had a single birthplace—its own 'Centre of Creation'—but he maintained that many species must have had several Centres of Creation, and creation was in his mind no figurative expression. He meant by it, just as Linnaeus before him had meant, a direct act of God; in other words his belief was that there had been going on around us a series of mysterious performances, not one of which had ever been consciously witnessed by a human eye, but each of which had for its object the independent formation of a new living being, animal or plant. That is to say that there had been going on from time indefinite a continuous series of operations which could only be termed miraculous, since there was no known natural law by means of which they could be produced. Though the author of this theory was, in the country of his adoption, regarded as the especial champion of opinions that are commonly termed orthodox, it is not surprising that many minds revolted from such a conclusion as it required—a conclusion which they not unfitly deemed a *reductio ad absurdum*. Yet the position of Professor Agassiz was perfectly logical when once his premisses were admitted; and, more than that, it became obvious to all clear-seeing men that one of these alternatives must be adopted—either Agassiz's logical doctrine of Centres of Creation, or the theory of the Transmutation of Species, which had been so long condemned because no reasonable explanation of its *modus operandi* was known.

I have called these alternative opinions because I believe that no third course had been suggested by any naturalist, and yet it is hard to say which of them was most unpalatable to the world at large. On the one hand people were called upon to believe that Man was in some inexplicable and unaccountable way produced from a Monad. On the other hand they were called upon to believe that the inhabitants, vegetable and animal, whether bestial or human, of nearly every group of islands in the Pacific Ocean were the result of innumerable special acts of Creation entirely performed within the limits of almost each cluster of coral reefs. The natural consequence of this was that most people, and even most biologists, remained in an apathetic if not an unthinking condition on this subject, and went on as their fathers had done, not caring to trouble themselves in this matter. It was only a few—an extremely few—among them who ever gave the question any consideration at all, and these few were not so much the men who had confined their labours to museums, libraries, or laboratories, but they were, with scarcely an exception, men who had studied Nature in the field, and had seen her works under varied aspects in the most distant and diverse climes. They were of the men who had personally compared the geological formations of the Old World and the New, men who had circumnavigated the globe, who had surveyed Antarctic volcanoes or Himalayan snows, who had dredged the depths of Australian oceans or had explored Amazonian forests. Out of the abundance of their observation and reflection these men—to this audience I need not name them—in due time delivered their verdict, and when it was delivered its effect was crushing. The position of the supporters of the doctrine of 'Centres of Creation,' logical as it had seemed,



was swept away—not of course without a gallant struggle on the part of its defenders—and the theory of the ‘Transmutation of Species,’ fanciful and unreasonable as it had been thought, was under a new name established, the very fact of its success being an additional proof of, to use Mr. Herbert Spencer’s happy phrase, the ‘Survival of the Fittest.’

But perhaps some of you have been thinking or whispering to your neighbours, ‘Why should our president be taking up our time by making us listen to all these platitudes, this old story with which we are all familiar?’ and if you have been so doing you will have some excuse, but I trust you will think that I also have some excuse in thus recurring to what may be almost deemed a portion of ancient history when I state that in my belief this year 1887 will in future be remembered as that in which ‘The Life and Letters’ of our great biologist, Charles Darwin, appeared; and I hope that in a few minutes you will admit that in accordance with the fitness of things it is meet and right that this should be so. There can be little doubt that before the end of this year that work which all naturalists have been expecting with so much anxiety will be published, and published moreover in three languages. It can hardly fail to be accounted by biologists as the chief event of the year. By favour of its author, Mr. Francis Darwin, I have been allowed to see some of his proof-sheets, and I am sanguine that it will not disappoint the expectations of its readers. On one point I venture to speak with some certainty. The noble character of the man will be made manifest to the world in words and deeds that cannot be spoken against, and we may feel assured that in future

Whatever record leap to light,  
He never shall be shamed.

He is unsparing of his own mistakes or shortcomings; and, while admitting with the utmost generosity the assistance he received from others, the dignified way in which he thought and expressed himself toward the many who attacked him, often unscrupulously and in a manner which he could not but deeply feel, will ever redound to his credit, and prove him to have been that great philosopher which all his friends and adherents would wish to believe him. Do not mistake me, however, in one respect; there were times when he ‘did well to be angry’; but his anger was slowly excited, and his occasional vehemence soon subsided into his wonted calm. More than all this, you will find that the childlike simplicity of his mind and the single-heartedness of his devotion to the study of Nature which characterised the beginning of his scientific career endured unto the end. His admission at the outset of “utter ignorance whether I note the right facts”; his confession that he was “nothing more than a lions’ provider”; his unfeigned astonishment at discovering that his early observations were of any worth—are all of a piece with the humility he subsequently displayed when his success was declared. As he found, one after another, many of his contemporaries and still more of the younger generation of naturalists adopting his views, his joy was great; but that joy was not alloyed by any feeling of pride. He did not care for making a convert to ‘Darwinism’—his exultation was that the strength of truth, of reason and of observation had prevailed. In the same lowly spirit he, when at the height of his fame, expressed his gratitude to those, whosoever they might be, that helped him in his labours; and, if I might be critical on this point, I should say that his inherent goodness of heart often caused him to exaggerate the importance of the help they gave. Not a spark of jealousy was kindled in his mind; and at what may be considered the most trying moment of all, when the theory he had for twenty years been testing by every means in his power, the theory on which he built all his hopes of future recognition, the theory which he not unnaturally believed to be his peculiar possession—when this theory, I say, was independently conceived by another naturalist, his conduct was emphatically that of a man of honour. It pained him acutely to think that this naturalist, a trusted correspondent, an esteemed philosophical observer, and at the very time a wanderer far from home, should be deprived of the full glory of his ingenuity; and, but for the counsel of judicious friends (whose good advice on this occasion is indisputable), Mr.

Darwin would have withdrawn every claim of his own to this great discovery, and left it entirely to Mr. Wallace! In the history of science and invention I think there are few cases like this. When you come to read the book you will find that though he unreservedly placed the matter in the hands of Sir Charles Lyell and of Sir Joseph Hooker, it was some time before he could reconcile himself to the notion that they were not unduly favouring him at the expense of his competitor. Such was the man! Though you are doubtless all aware of the fact, it would be wrong in me if I omitted to remind you that Mr. Wallace's conduct under these circumstances—sufficiently disappointing, as all must admit, to him—was in every way worthy of Mr. Darwin's. If in future you should meet with any cynic who may point the finger of scorn at the petty quarrels in which naturalists unfortunately at times engage, particularly in regard to the priority of their discoveries, you can always refer him to this greatest of all cases, where scientific rivalry not only did not interfere with, but even strengthened, the good-feeling which existed between two of the most original investigators.

I said but a few minutes since that it was fitting that the memoir of Mr. Darwin should appear this year—this year of jubilee—and a very remarkable anniversary I now have to point out to you. I learn from the Memoir that Mr. Darwin's pocket-book for 1837—just fifty years ago—has this entry:—

“In July opened first note-book on Transmutation of Species. Had been greatly struck from about the month of previous March on character of South American fossils, and species on Galapagos Archipelago. These facts (especially latter), origin of all my views.”

Other passages in his already published works confirm this memorandum; but we had not hitherto known with certainty when the views originated. We may now, therefore, celebrate among other jubilees that of Mr. Darwin's adopting the theory of the Origin of Species by Natural Selection, though I am bound to tell you that it was not until a few months later—about the beginning of 1838—that, after reading Malthus's work, the full conviction of the truth and sure ground of his speculative views came upon him.

I would not have my audience disperse with the impression that my business here is merely to point out what has been done by the genius of the great man of whose character and labours I have just been speaking. Enormous as are the strides which he has enabled us to make, you will all admit that it behoves us to follow in the paths he has indicated. We may depend upon it that what we know bears a very small proportion to that which we do not know, and I venture to recall your attention to that subject, which, as I have just said, was the origin of all his views. That subject is the Geographical Distribution of animals and plants, not only at the present time, but in bygone ages. As regards Botany, I do not dare in the presence of so many distinguished authorities to say more than this—that I believe the greatest and most important results of their labours in this direction are inadequately known to zoologists, while in Zoology I am certain that there are many large groups of whose distribution we are almost entirely ignorant.<sup>1</sup> That excellent work has been done in some groups all will admit, and in regard to the difficulties which have precluded the investigation of the subject in other groups I am well aware. But not only do we need further investigation in regard to them, we want much more correlation of results than we yet possess, and still more a comparison of the results obtained by botanical and zoological enquirers. Here there is a wide field, and a field worthy of cultivation. I do not know that a more competent body of cultivators can be found than within this section of the British Association, and if they can be persuaded to make common cause, the study of Biology will be much advanced. We have been told that it is as useless to investigate the origin of life as the origin of matter. That may be true or it may not; but it seems to me that to

<sup>1</sup> I say this after having studied Professor Heilprin's recent work, *The Geographical and Geological Distribution of Animals* (International Scientific Series, 1887)—in many respects the fullest on the subject—and also Mr. Helmsley's admirable *Introduction to the Botany of the Biologia Centrali-Americana*, which will shortly appear. The opportunity of reading the latter I owe to the kindness of Mr. Salvin.



learn the way in which life has spread over the globe ought to be within the capacity of man, and we can hardly learn that way except by far more intercommunication of special knowledge than has hitherto been made. It is evident that with the existing minute subdivision of biological research the subject is beyond the power of any one man; but I should rejoice if anything I could say on this occasion might put in train some alliance between Botanists and Zoologists for the object I have just suggested. It may be said that we have not sufficient information as to certain parts of the world to enable such an alliance to effect its work satisfactorily. If that be the case I am sure you will join with me in thinking that these insufficiently-known parts of the world should be subjected to a thorough biological exploration. For many years past I have been accustomed to hear an adage that "Property has its duties as well as its rights." If I am strongly in favour of the rights of property, I am no less prepared to exact from it its duties. Various events have given to this nation rights of property in many parts of the globe. I think we ought to justify those rights, and there is no better way of doing this than by performing the corresponding duties. It is incontestable that among the dependencies of the British Crown there are innumerable places—*islands*, large and small, territories the limits of which no geographer or diplomatist can define, and so forth—of which the fauna and flora have never been scientifically investigated. It is right, of course, that I should recognise the successful efforts made in many instances by the directorate of the Royal Gardens at Kew, and to a less extent by private persons. But why should not a properly organised biological investigation of all the portions of the empire be made? You will, I think, all agree that it is our duty to carry out investigations of this kind. Whether they would be better performed under the superintendence of Her Majesty's Government or not is a point on which I reserve my opinion, only mentioning that the success which has attended those instituted by the botanical authorities at Kew leads me to suppose that an extension of the method there followed might produce results as satisfactory; but, if this be the course adopted, I must point out that the organisation of a corresponding zoological and geological directorate will be needed. This matter I merely throw out for your consideration; but I would add that if anything is to be done no time is to be lost.

When on a former occasion (at Glasgow in 1876) I had the honour of addressing a Department of this Section, I pointed out the enormous changes that were swiftly and inevitably coming upon the fauna of many of our colonies. The fears I then expressed have been fully realised. I am told by Sir Walter Buller that in New Zealand one may now live for weeks and months without seeing a single example of its indigenous birds, all of which, in the more settled districts, have been supplanted by the aliens that have been imported; while further inland these last are daily extending their range at the cost of the endemic forms. A letter I have lately received from Sir James Hector wholly confirms this statement, and I would ask you to bear in mind that these indigenous species are, with scarcely an exception, peculiar to that country, and, from every scientific point of view, of the most instructive character. They supply a link with the past that once lost can never be recovered. It is therefore incumbent upon us to know all we can about them before they vanish. I have particularly instanced birds because I happen to have studied them most; but pray do not imagine that the same process of extirpation is not extending to all other classes of animals, or that I take less interest in their fate. The forms that we are allowing to be killed off, being almost without exception ancient forms, are just those that will teach us more of the way in which life has spread over the globe than any other recent forms, and for the sake of posterity, as well as to escape its reproach, we ought to learn all we can about them before they go hence and are no more seen.

I have just now applied to these expiring forms of New Zealand the epithet ancient, and in connexion therewith I would, by way of conclusion, offer a few remarks on the aspect which the subject of Geographical Distribution presents to me. Some of us zoologists—I am conscious of having myself been guilty of what I am about to condemn—have been apt to speak of Zoological Regions as if they were, and always had been, fixed areas. I am persuaded that if we do this we fall

into an error as grievous as that of our predecessors, who venerated the fixity of Species. One of the best tests of a biologist is his being able to talk or write of 'Species' without believing that the term is more than a convenient counter for the exchange of ideas. In the same way I hold that a good biologist should talk or write of 'Zoological Regions.' The expression no doubt arose out of the belief, now scouted by all, in Centres of Creation; and, as sometimes used, the vice of its birth still clings to it. To my mind the true meaning of the phrase 'Zoological Region' is that of an area inhabited by a fauna which is, so to speak, a 'function' of the period of its development and prevalence over a great part of the habitable globe, but at any rate of the period of its reaching the portion of the earth's surface where we now find it. One great thing to guard against is the presumption that the fauna originated within its present area and has been always contained therein. Thus I take it that the fauna which characterises the New-Zealand Region—for I follow Professor Huxley in holding that a Region it is fully entitled to be called—is the comparatively little changed relic and representative of an early fauna of much wider range; that the characteristic fauna of the Australian Region exhibits in the same way that of a later period; and that of the Neotropical Region of one later still. But while the first two Regions have each been so long isolated that a large proportion of their fauna remains essentially unaltered, the last has never been so completely severed, and has received, doubtless from the north, an infusion of more recent and therefore stronger forms; while, perhaps impelled by the rivalry of these stronger forms, the weaker have blossomed, as it were, into the richness and variety which so eminently characterise the animal products of Central and South America. I make no attempt to connect these changes with geological events, but they will doubtless one day be explained geologically. It is not difficult to conceive that North America was once inhabited by the ancestors of a large proportion of the present Neotropical fauna, and that the latter was wholly, or almost wholly, thrust forth—perhaps by glacial action, perhaps by the incursion of stronger forms from Asia. The small admixture of Neotropical forms that now occur in North America may have been survivors of this period of stress, or they may be the descendants of the more ancient forms resuming their lost inheritance. Beyond the fact that these few Neotropical forms continue to exist in North America, its fauna seems to be in a broad sense inseparable from that of the Palæarctic area, and, in my belief, is not to be separated from it. The most difficult problems are those connected with the Ethiopian and Indian (which Mr. Wallace calls the Oriental) areas; but I suppose we must regard them as offshoots from a somewhat earlier condition of the great northern or 'Holarctic' fauna, and as such to represent a state of things that once existed in Europe and the greater part of Asia. To pursue this subject—one of most pleasing speculation—would now be impossible. I pray you to pardon my prolixity, and I have done.



ADDRESS  
TO THE  
GEOGRAPHICAL SECTION  
OF THE  
BRITISH ASSOCIATION.

BY

COLONEL SIR CHARLES WARREN, R.E., G.C.M.G., F.R.S.,  
F.R.G.S.,

PRESIDENT OF THE SECTION.

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‘The geographer should therefore chiefly devote himself to what is practically important.’—STRABO, c. i. § 19.

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My predecessors in former years have used their discretion in the opening address either to generalise on the science of geography or to lay stress upon those particular subjects to which they considered it desirable to call attention. I propose on this occasion to refer to matters which have long been of importance to those who are desirous of the spread of the knowledge of geography, and in which I trust the public generally are acquiring an interest. I refer to the teaching of geography in our schools and the economy and advantage to the State which would result from a more perfect and skilful system of instruction.

The term geography covers a very wide area, and while limiting its use to-day to the more restricted sense generally accorded to it in modern times, I must protest against its being applied only to a dry digest of names of places and record of statistics, rendering it a bugbear in the instruction of youth instead of allowing it to cover all those interesting and engrossing subjects which truly belong to it, and without the knowledge of which the mind of youth cannot be trained and expanded in the direction to which the science tends.

As the geographer Strabo points out, our science embraces astronomy, natural history, and is closely connected with meteorology and geometry, the arts, history, and fable; but since his day so much progress has been made in the arts and sciences that the branches of geography have become specialities to be taught separately, and the old root geography has been almost laid aside and treated with contempt, though it is only by a thorough acquaintance with it, the knowledge of common things, that the branches which depend upon it can be thoroughly comprehended. We may take geography, then, to embrace all that knowledge of common things connected with the surface of the earth, including the seas and the atmosphere, which it is necessary for every human being to be acquainted with in order that progress in other knowledge may be acquired and acquaintance with the world be made which will fit man for life in any capacity, whether as occupying the highest position even to the most humble. Indeed, it is difficult to say in what capacity in life this knowledge is most required. No man can do practical work without it, and to the theorist it is absolutely essential.

The science may be divided under two heads: that which we learn from others, that which we acquire from our observation and researches. All experience tells us that the information is most valuable which we acquire by our own exertion, and therefore every effort should be made by those interested in the welfare of mankind to endeavour that each one should learn everything that can be learned from his own observation properly directed.

Year by year, as the surface of the earth becomes better known, the districts in which explorations of an adventurous nature can be made diminish more and more, and as scientific research takes the place of that of a ruder nature the chances of excitement grow perceptibly less. Indeed, when we look upon the knowledge possessed by the ancients and study their cosmogony we cannot but feel the loss we have sustained in approaching the truth. The poetic halo with which everything was encircled, the deep shadows and gloom, have gradually been dispersed and dispelled, together with all the distant and uncertain light which gave so much scope to the imagination, and we now view the hard stern realities of fact, brilliant and gay in their colouring, but leaving no room for fancy, or for a change of ideas—always the same vivid rigidity of outline which admits of no two opinions. It is like the change of scenery from our own beautiful cloudy island, where the tints and shades change from hour to hour, and where the grey and purple distances leave so much to the imagination, to the bright scenes of the Mediterranean shores, where everything is bathed in intense sunlight, and distinctness of outline reigns supreme, where there is no possibility as to doubt.

In each case we may balance the advantages and disadvantages; but as we have gained in knowledge so we are losing in understanding. We are fast losing our human nature and are becoming machines, and we call it being civilised. We are drifting into a condition in which we learn nothing of ourselves or by our own individual efforts, when to act honourably if it is against our interest is considered the act of a fool; we are coming to a time when, as we know more about science, and are better educated in arts, we know less about mankind, and are the less able to assist in gaining knowledge for the world; all power of doing so is day by day becoming vested in the hands of a few scientific men, on whose word we have to rely. In this progress we are losing all we used to hold most dear; the desire of living for others is departing, and with it hospitality, chivalry, enthusiasm, unselfishness, and because we are unable to receive the talents given to us they rust and corrode. No doubt we are able to seek other channels for our energies of mind, but how are we to exert our physical powers for the benefit of man? In days of yore it was open to any man of spirit and strength and activity to set out in quest of adventures of the unknown for the assistance of his fellow-men, to relieve the world of its monsters, to risk everything for others. But those days of daring are now gone by; the doubt, uncertainty, and mystery attached to unknown danger are no longer to be met with, and though the same chances are always presented to human nature to practise self-denial, they are now, though more difficult perhaps, of a passive instead of an active nature, and do not so distinctly belong to the domain of geography as they did in olden times.

As the people of olden times are to those of the present day, so may we consider the child to the man; and we adults in this assembly must recollect that, however strong may be our emotions and passions at the present time, they are but of a mild and vapid nature when compared with the aspirations and feelings of youth. Each prosaic-looking child is full of poetic and romantic feeling, to which as a rule utterance is never given, but which, nevertheless, cannot be rudely shattered without injury to the mind, and which, if taken advantage of, may assist greatly in training the mind and developing a love of geography.

It should be a matter of great interest to those who instruct in geography to study its gradual development from the earliest data and to watch the progress it has made. And this is not a matter of very great difficulty, for as geography is the knowledge of common things, and the ancients were more experienced observers than ever we may hope to be, the earliest records we possess are full of geographical accounts. In the books of Moses, three thousand years ago, we obtain our first recorded view of the cosmogony of the ancients, at which time the world is sup-



posed to be a flat disc with water surrounding the land, and this idea pervades later books, and is dwelt upon in the Psalms of David. Homer also held a similar view, and to him is accorded by Strabo the honour of being the founder of geographical science, because he excelled in the sublimity of his poetry and his experience of social life; and the reason why he excelled is carefully related. He could not have accomplished it had he not exerted himself to become not only acquainted with historical facts, but also with the various regions of the inhabited land and sea, some intimately, others in a more general manner. 'For otherwise he would not have reached the utmost limits of the earth, traversing it in his imagination.' Herodotus, to whom we are indebted for furnishing us with the *earliest known system* of geography, also held the same view concerning the earth; but it is worthy of remark that he speaks in his day (450 B.C.) of there being another view, as to the world being round, which he considers to be exceedingly ridiculous, and therefore it may be surmised that even at that early period there were minds that had arrived generally at the conclusion which now obtains as to the shape of the world. The idea that the sun, moon, stars, and planets revolved round the earth was the view in early days, and continued up to quite a recent period, and even now we are unable to prove that the generally received system is correct, and only use it as being more convenient than that which makes the earth the centre of the universe.

When we come, however, to consider the progress of discoveries on the surface of the earth itself, the strides in latter years appear to be enormous, but yet we must not forget that there is an ebb and flow constantly going on. Discoveries are made and lost sight of, and again are brought forward as new. Sometimes after an account of discoveries has been published a second account differs most materially from the first, and the public have to wait for further examination. Cases have occurred, as in the early Portuguese discoveries in Central Africa, in which the plans and accounts have been laid on one side and forgotten, and the territories rediscovered and surveyed years afterwards. Again, sketches of new countries have been made, and the surveyor has omitted to show what is conjecture and what is from actual observation, and his plans throughout have been discredited. In some cases these mistakes have retarded discovery, in some they have directly led up to it—as, for example, in the gigantic geographical error in placing on the globes of the fifteenth century the eastern extremity of Asia no less than 150 degrees longitude too far east, which prompted Columbus to endeavour to reach Asia from the west, and thus led to his discovery of America.

In gauging the progress of our knowledge of geography we must not, however, simply take into account what has been made by ourselves, but by the known world generally; for example, although the Portuguese circumnavigated the Cape and proved that it was practicable to do so, it is still a moot question whether they were attempting what was known or unknown. At any rate it seems certain that in the thirteenth century—not to go back earlier—the Arabians were aware of the fact that Africa on the south was surrounded by the ocean, and the geography of *Abulfeda* clearly points this out.

It is, then, a difficult matter to decide what is a discovery in geography. We may possess an exact description of a town and know its position, and yet it may never have been visited by a traveller from what we term civilised Europe.

What we require, however, is precise and accurate information of the earth's surface, however it may be obtained, and to train the minds of our youth in the powers of observation sufficient to enable them to obtain this information; and if in so doing our countrymen continue to be stimulated to deeds of daring, to enterprise and adventures, to self-denial and hardships, it will assist in preserving the manhood of our country, which is more and more endangered year by year, in consequence of our endeavour to keep peace within our borders and to stave off strife with our neighbours.

Probably many of us here to-day of mature age, on looking back at our early acquaintance with geography, will recollect little but a confused list of proper names and statistics, learnt by rote, and only imperfectly carried in the mind, so that only a few portions stand out still visible, and those probably

connected with pleasurable and, in some cases, painful accessories; perhaps those particular lessons which we may have assisted some school friend to master still remain as clear as ever; or, again, those learnt under the terror of the rod.

Taking schools and subjects all round, nothing probably has ever been worse taught than geography was only a few years ago, and very little progress towards a good system has even yet been introduced into higher class schools, though in the schools of the people an effort has been made to render the subject more palatable and instructive.

The faults, however, of the system hitherto in use are now fully recognised, and objections are general that the study has been made too painful a grind and that the whole process has been of too severe a character. If this were the only fault to be found in the old method, I for one would be inclined to adhere to it, assured, as I am, that no training of the mind can take place without great denial and sacrifice in learning self-control. But the real question is as to the practical results of the old system. Are they of such a character with all or the majority of minds (of all classes and conditions) that they have become stored with useful knowledge and at the same time trained to take a pleasure in increasing it in the future? If the results are short of this we cannot but pronounce the old system to be a failure, as the knowledge of geography is the knowledge of common things inseparably connected with the life of each one of us, and there is no better medium through which the mind can be trained to be always in a condition for acquiring knowledge without making too great an effort.

Unfortunately for the prospects of introducing a complete and perfect system of teaching geography (suitable to most minds), the reaction that has set in recently is likely to lead to evil results if not carefully curbed. It seems now to be desired to promote the acquirement of knowledge at the earliest age without effort and without hard work; but this appears to be directed towards alleviating the toils of the instructor as much as the instructed, and we have now, as a result, children taught common things without any effort to strengthen their memories, and then a system of cramming introduced at a later period, when the memory has ceased to be capable of responding to the efforts made, and consequently all the information crammed in is dropped again in a few months.

The memory of youth is like a cup swinging freely on a pin thrust horizontally through its sides. If the pin is below a certain line, the cup will tilt over and lose its contents when filled up beyond a given level; but if the pin is near the upper edge the cup can be filled with more and more security. By careful training in the earliest years the cup may be constantly kept full in later years; but by the training at present in use the cup tilts over far too soon.

It seems to me that the remedy recently adopted is worse than the disease it was to eradicate, and that however injurious it was to attempt to store the mind with mere names, yet the memory was trained thereby to retain something definite; and it is still worse to attempt to store the mind with mere ideas without the connection of names, and leave the memory to rust.

There is obviously a middle course which may rid us of the errors of the past without leading us into still greater difficulties. And if we keep the object to be gained always in view, we cannot fail to take a direct line. We want first to lead the memory to constant exertion of such a nature that it grows stronger day by day, but is not overstrained or wearied; at the same time it must be stored with useful facts, which may be quite above the capacity of the mind to comprehend at the time, but which will be required all through life: this can readily be done by means of verses or rhymes set to simple airs and committed to memory by song. There are facts of the greatest importance which can be learnt in this manner with very little effort, and which, if not fixed in the mind at a very early age, the want of them may be felt throughout life.

As, for example, the directions in which latitude and longitude are reckoned, in which the sun rises and sets, the relations of the east and west respectively to the north and south, and many other matters which appear to be of a trivial character, but which require to be as rigidly committed to memory by rote as does the multiplication table.



These very small matters are the foundations of everything we require to know, and if we do not have these foundations firmly and securely fixed, we will be the sufferers all our lives. Too much attention cannot be paid to them, as it is the early lessons which remain most clearly fixed in our minds.

A point connected with this subject, which admits of much discussion, is as to *how* such verses should be learnt, whether with the assistance of books, pictures, or metaphor. Should they come to the memory through the eye, or the ear, or through both? As a beginning, I think that geography should not be learnt from books, but from the teacher, who may use diagrams and pictures, but at the same time text-books should not be done away with, as is so constantly advocated; on the contrary, they should be adhered to most rigidly. There are few teachers who could improve on a good text-book, but these books should be for the teachers, and not for the children.

Children have a remarkable capacity for making pictures for their mind's eye of every thing they think of, which is dulled gradually as books are taken into use; this faculty, if made right use of, may be developed, and will greatly assist the study of geography, and will lead to a 'picture memory,' which will be most useful in regard to maps, drawing, and spelling. This faculty can, of course, be over cultivated, but there is not the remotest danger of this occurring at present in any of our schools. When highly developed, we find it employed by novelists, who can bring their characters up before them and picture them enacting their parts, and also by artists, who sometimes lose the power of discriminating between that which they actually see and that which their picture memories call up.

Although it seems to me absolutely essential to cultivate and develop the memory, so often called the 'parrot memory,' of young children, this is by no means all that is necessary. At the same time must be taught the proper use of the powers of observation with reference to nature, which in towns is so difficult a matter, placing the bulk of our population at so great a disadvantage. One of the first points neglected by teachers generally is to explain to children what the object or result of the lesson is to be. In most minds it is very difficult to pay real attention unless it is known what is to be the general drift of the conversation, for otherwise the mind will be directed to points quite irrelevant. Children should be first told in a few words the line the lesson is going to take; this will greatly tend to secure the attention of what are termed dull children, who often, if properly treated, would turn out the cleverest, but who cannot grasp a subject until they see it from all sides, and know it thoroughly, while the 'clever children' are satisfied with a view of one side only. The foundation should be laid slowly, the progress being governed by that of the 'dull children,' who often will most amply repay the teaching. The clever child will not be hurt by having the subject impressed upon his mind over and over again, so long as it is made interesting.

Great care must be taken in the method of presenting maps at an early age before children, and a distinct idea should be given of the difference between a map and a picture.

It must be recollected that from the moment geography is taught, children will make maps or pictures in their mind's eye, whether they are actually presented to them or not.

For example, if a house or a garden is mentioned, both the teacher and the child must view it from the outside and from a certain distance, for it is impracticable for most minds to look all round and behind at one time. To have a full view of what is mentioned it is necessary to get outside and beyond it. Children will differ among themselves in their method of viewing what is spoken of, but the teacher can readily ascertain what mental pictures they have formed, and can make use of this faculty in the first use of maps. Children should first be instructed in maps of the village or town in which they live. It is remarkable how readily uneducated nations in uncivilised countries can understand plans from their constant observation of nature. Most intelligent Bedouins are able to make a rough plan or diagram in the sand with a stick of the district they know, and will also take care that the orientation is correct. Kaffirs can do the same, and can point out the direction of a cattle post fifty or sixty miles distant with unerring sagacity.

It is of vital importance that children in our island, who cannot under ordinary circumstances have sufficient opportunities for using, cultivating, and developing their powers of observation to any purpose, should have the use of maps put before them in such a manner that they will not be led into error. Otherwise they will have fixed in their minds factors of discord which the teacher may know nothing of, and which will trouble them through life, and which if they do get rid of with great labour in after years will constantly return at unseasonable moments.

It is very common for children to mistake east for west, north for south, and even to make still more ridiculous errors which appear on reflection to be quite impossible. Yet these errors remain often unobserved until the youth is eighteen or nineteen years old, when he begins to think the matter out for himself, from finding that he is continually making absurd mistakes, but then it is too late for him to do more than know that he is liable to the error, for on an emergency it will crop up in spite of himself.

I am aware of one instance in which an educated surveyor when thinking of London invariably placed the portions about Regent Street and Charing Cross in an inverted position while picturing all the rest correctly, and it was only by an effort that he could turn this portion upside down into its place. Another, when thinking suddenly of Paris, always placed it to the north of London, and another always thought of the west end of London as being towards the eastern coast.

Out of thirty cases of well-instructed men at an age between eighteen and twenty, I have found that about eighteen were under the impression that while the sun rises in the east, the stars rise in the west, from having learned that the sun has a proper motion among the stars.

I fancy there are few educated men who have not grown up with some curious errors with reference to geographical facts which have bothered them all their lives, and which they have found it impossible to get rid of even when they have discovered where the errors lay, and I believe that many of the numerous blunders and accidents which constantly occur on railways, with shipping, machinery, &c., and the causes of which cannot be accounted for, are really to be ascribed to some early error in learning geography or the knowledge of common things, errors which, when attention and watch over self is suddenly withdrawn, influences the actions in a contrary direction to that which is right.

As an instance of the natural liability to error, even apart from those which may be ingrained while under instruction, I may allude to the feeling when the eyes are shut when travelling by rail or carriage that the vehicle is going in an opposite direction to that in which it actually moves, to the impression when approaching or leaving land in a boat or balloon that the earth is moving and that oneself is stationary; even when on horseback under excessive fatigue in the dark the traveller has been known to imagine that the horse was moving rapidly backwards. The effect of excessive fatigue from physical exertion has somewhat the same result as a want of self-control from bad training of the mind, and perhaps those who have ridden for many hours on horseback or in a coach may have noticed how in the dark a fixed lamp may be seen to make various fantastic signals due to the motion of the horse or coach transferred by the eye to the lamp. As another instance of the difficulty of self-control I may mention a case in which a man well instructed in taking astronomical observations and in the rudiments of astronomy could not divest himself of the idea, which he had gained as a child, that the moon shines with light of her own, and that her phases are due to the earth getting between her and the sun, this error continually interfering with his mental astronomical pictures, though when his attention was specially called to the subject he was aware of the error which intruded itself so constantly in his views of the heavenly bodies. The difficulties regarding east and west, north and south probably arise from a multiplicity of causes, such as the southern side of the Mediterranean being the northern coast of Africa, or the southern view of a house being obtained by looking towards it in a northerly direction, and these difficulties as to orientation do not only occur in modern times but are to be found in ancient writings. Another constant source of error is inverting names unconsciously, such



as speaking of Jupiter's rings and Saturn's belts. As an instance of this I mention a case in which, a lecture being given on the Franco-Prussian War, the lecturer inadvertently in the middle of his lecture used the word 'Prussian' for 'French,' and *vice versa* continually throughout, and though he was quite aware of some anomaly every now and then, he could not ascertain where he was in error until near the end of his lecture. Another source of error which cannot be too carefully guarded against results from placing the celestial globe by the side of the terrestrial globe and treating them as though they are of the same character; this is certain to confuse east and west with most children, as one has to be looked at from the outside and the other from the inside in actual fact. Again, as some star charts are made that they may be looked at from above and others from below, causing the east and west points to differ, there is sure to arise confusion. I venture to say that there are few young minds which are not absolutely and hopelessly confused by the use of celestial globes and charts. I believe it to be essential that until the mind is fully trained and developed the stars should be looked at from within and not from without, and it appears to me that all the information which a child can require, apart from practical observation, concerning the phenomena of day and night, the seasons and months, the circles and zones, the phases of the moon and eclipses, can be imparted by the use of a lamp with a reflector and two globes, though a good orrery placed in the school for children to examine and observe for themselves would often enable the dull ones to keep up with the rest more easily.

It will be interesting to note whether the class of error alluded to does not arise principally among those bred in towns, and who have not had an opportunity of developing their observation in the country; as with those who do use their observation a habit is acquired of unconsciously working out questions which arise, and the mind arrives at a correct conclusion. This end should be the great aim and object in instructing in geography, for as there is no royal road to knowledge divested of grind and pain, there is yet the path which provides the greatest amount of result with the least amount of grind, in which all the labour expended is productive, and in which after a time labour even becomes a pleasure.

It seems very desirable that the first maps presented to a child, viz. those of the school grounds and the parish, should be placed on the floor and properly oriented; this will go far to fix the correct positions of east and west, north and south, and will prevent the idea of the north necessarily being *up* and the south *down*. It is to be observed that if the child looks up to a map it is almost equivalent to looking at the map when lying on the back, in which case the east and west are inverted. The motion of the sun over the map might with advantage be pointed out at various times of the day, and if the position of the rays of the sun on the floor when on the meridian could be shown each day when practicable on the line drawn north and south, it would do much to fix in the mind the fact that the sun is in the meridian at apparent noon each day. A sundial should also be available in every school-yard to which children may have access.

The map of the district round the school should only be made use of in order to clear the way to understand what a map is, for reference in describing other maps and for practical purposes in giving the child useful information as to the places in the neighbourhood. While this is going on the child should be taught to point out the directions of the principal towns, &c., in the county and island, and then an outline map of the British Isles with the principal places and features marked on it should be brought under review. Too much detail should not be crammed into the early lessons; a good firm foundation is required, something to start upon before the great test of faith is made in teaching, viz. that the world is round.

Children should be taught, as far as is practicable, to make this discovery for themselves, and many will arrive at it one way or another, or think they do so, which is equally important. It is far better they should grasp truths themselves than have them drummed into them; it gives them confidence in their own deductions and leads to further observation of nature. In introducing the world as round, a *blackboard* globe should be used, about three feet in diameter, on which the continents are outlined boldly in red, with some meridians and parallels of latitude

in white. It would be well if a portion of this globe could be taken to pieces to show how a horizontal sundial for the particular latitude is constructed, and for other matters of interest. It is material to show that the earth revolves on a fixed axis from day to day, and in one direction. All the great difficulties in learning geography are at the threshold of the science for those who have not observed nature; the more abstruse subjects are comparatively easy to teach.

The first difficulty common to all is that regarding latitude and longitude, regarding which there are so many elements of error. It is so difficult for the child to recollect which term means length and which breadth, and then to get the restive imagination to grasp the fact that the length is sideways and not up and down, as it apparently should be; for even if the earth is shown to be an oblate spheroid, there is nothing to lead a child to see that there is a greater circumference round the equator than round the poles, and the time has not arrived to perplex the child with the views of the ancients on the subject. Then, again, if the child does recollect that the meridians of longitude run from north to south, and the parallels of latitude from east to west, it is probable that he may measure the longitude in degrees along the meridian and the latitude along the parallels; a very common and recurring error, difficult to deal with. The only practicable method is to put the facts of the case into amusing verse and commit it to the memory by song. At this stage, also, some easy standards of measurement put into verse and to music should be learnt by rote, to enable the child readily to recollect the relative measurements of the earth, sun, and moon, and the radii of their orbits and times of progression.

I lay great stress upon these matters at the beginning, because they are really all in all to those who wish to succeed in the science in after-life, and I have viewed the matter from the standpoint of what will be required at the age of eighteen to twenty, when the mind ought to be capable of taking up any subject, instead of considering what show of learning the child should be able to produce in an examination at an early age. The stock-in-trade of knowledge for each young person need be very slender, but it must be of the right sort and best quality. No doubt there are many children badly trained who can gradually work out matters correctly for themselves, but these are the few with originality of mind, and even they would be benefited by not having to spend a portion of their lives in unlearning.

Once the preliminary difficulties are over and the power of observation and contemplation is acquired, even in a small degree, the study of geography becomes but a simple matter, for it is the learning of common things, matters of everyday life, which we may, if in the country, acquire to a partial extent of our own experience; but though so simple it requires continuous application and attention.

In each calling or trade a man may become an experienced geographer to a limited degree. The pilot, for example, is an expert in the geography of the seas he works on, for he not only knows the ports, the coast lines, and the sunken rocks and sandbanks, but he also knows the tides, the winds, he studies the clouds and the currents, and has an intimate knowledge of the contours of the shallows; moreover, he knows the shipping of various countries, the merchandise they carry, and the produce shipped from each port. In the same manner, by hunting, shooting, fishing, bicycling, birdsnesting, &c., we acquire a knowledge of natural history and topography which aid us most materially in the study of geography, and which in a limited degree is the study of geography.

The bulk, however, of our children are cooped up in towns and walled playgrounds, and even when in the country are too often confined to one field; they have few opportunities of insensibly studying the wonders of nature, and therefore in order to develop their powers of observation and to understand geography artificial means must be made use of. Great efforts are now being made under the new code to produce these artificial means, by raised models and water and other devices, and it is to be trusted that if these schemes can be carried out the habit of observation will be induced; but the memory also must be at the same time actively exercised and stored with fresh facts day by day.

Even in large towns it is practicable to learn lessons in geography from actual experience and observation, for if the markets and railway produce are examined,



it can soon be ascertained from whence the articles come and from what ports, and with careful attention most valuable lessons in political economy can be gained.

The knowledge of geography thus, even in its restricted sense, embraces the life of an Englishman of every class and occupation, and its study is of the greatest importance to every man who has an occupation; it is singular that so little comparatively is thought of cultivating the science, and how small interest the State has hitherto taken in fostering this class of education.

But while the Board and other schools for the people are gradually taking up the work and endeavouring to work out a good system of education, it is mortifying to find how little progress has been made in the higher class schools where such heavy fees are charged; and the question arises whether in these schools the teachers of geography really understand the subject they teach, and would pass an examination before a Government Inspector.

The boys of the wealthy classes are put to the greatest disadvantage with regard to the study of geography. The son of a labourer will hear the price of provisions and clothing constantly discussed, so also with the son of a mechanic and tradesman, and will learn much about geography on the subjects with which the parents are connected, and will also in some measure learn to exercise his observation; but the son of wealthy parents is too often carefully kept from hearing all that might teach him geography, and he is seldom obliged to exert himself to use his observations in any essential matters of daily life; this is reserved for the playground, where nothing of real importance is at stake, and must have the most deleterious and detrimental effect on many young minds, and naturally results in so large a proportion becoming useless for any occupation.

It is apparent that, as education throughout the country progresses, the sons of the wealthy classes, if they are to compete successfully with others, must have some better mental training than they obtain at present, otherwise they will in a few years be distanced by the sons of the labourers, artisans, and shopkeepers. What an Englishman asks for is a fair field and no favour, and it seems hard upon a parent who struggles through life to make money to be enabled to give his children the best and most expensive education the country affords, that with it he must risk a training of the mind which is inferior to that in the less expensive schools of the people. As we are behind the Continental States and our colonies in so many of our institutions and land laws, so we are behind them in our training of the mind in our upper-class schools; by neglecting by artificial means to develop the power of observation among boys, who until they are put out in the world are never accustomed to do anything that will tend directly to any practical and useful result, we are putting them to the greatest disadvantage, and handicapping them in the race of life.

We omit to train the memory in early years, to lay a foundation of facts in the mind, and to develop any power of observation; we carefully prevent their doing anything useful, and bring them up in a moral atmosphere in which the idea of anything but amusement is practically excluded, and then in later years we attempt to adjust all our errors by cramming, when the memory is incapable of being crammed, and the mind has ceased to desire to acquire information; the result is that so many young men are deliberately rendered unfit for work in life, and those who have sufficient courage and energy to look their prospects in the face find the enormous disadvantages to which their teaching has subjected them, and lose precious years in unlearning and learning again.

More unfortunately still, the best and choicest of our minds cannot be crammed; and thus drop out at our examinations many minds of the class that for practical purposes would be most useful to the State. I allude more particularly to the minds endowed with contemplative faculties, which tend to originality and research; these minds cannot be successfully trained unless combined with the teaching there is something useful to do. It is often observable that an indolent, inert, and lazy boy suddenly becomes filled with enthusiasm and emulation, both at studies and in the playground, when subjected to a change of training. I venture to assert that every year at our public examinations many men are

rejected who are of the most superior class of mind for all practical purposes, who are physically most capable, who are so constituted that they cannot cram, and who, though retarded by want of proper training, are beginning to train their minds for themselves, and who if brought up under a good system in early years would take the highest places in examination. We are thus losing year by year from our front rank the men who would be of the greatest service to the State.

The pleas given for the study of geography by Strabo are worth bringing before the mind of youth, for he points out that while the success resulting from knowledge in the execution of great undertakings is great, the consequences of ignorance are disastrous, and he refers, among other instances, to the shameful retreat of the fleet of Agamemnon when ravaging Mysia, and to bring it more home to our everyday life he says: 'Even if we descend to such trivial matters as hunting, the case is still the same; for he will be most successful in the chase who is acquainted with the size and nature of the wood, and one familiar with the locality will be the most competent to superintend an encampment, an ambush, or a march.'

He further calls attention to 'the importance of geography in a political view. For the sea and the earth on which we dwell furnish theatres for action; limited, for limited action, vast, for grander deeds; but that which contains them all, and is the scene of the greatest undertakings, constitutes what we term the habitable earth; and they are the greatest generals who, subduing nations and kingdoms under one sceptre and one political administration, have acquired dominion over land and sea. It is clear, then, that geography is essential to all the transactions of the statesmen, informing us as it does of the positions of the continents, seas, and oceans of the habitable earth.'

Of all persons who require a knowledge of geography stand first those who are most concerned in the government of our empire, and yet, as has been mentioned, they have for the most part been brought up at schools where the mental training for geography is most defective. Our statesmen as a rule have neither theoretical teaching nor practical experience, and it is perhaps not too much to say that, putting on one side those who are merchants and sailors, there are no more ignorant persons with regard to geography than our law-givers. This ignorance endangers the safety of the country, for the people are continually perceiving, with regard to matters of everyday life and practical experience, that their law-givers are more ignorant than themselves, and are consequently continually interfering and giving advice in the details of the administration of the empire.

The progress and development of a free country depends upon the characteristics of the inhabitants, but these again depend in great measure upon the natural resources of the country—the soil, climate, mineral wealth, navigation, mountain ranges, risks and dangers from natural causes, and we must not omit the position of the country both with reference to commerce and war.

It is not usually the country too greatly favoured by nature which develops most rapidly, neither is it necessarily a long term of peace which favours progress; on the contrary, all experience shows that man requires a certain amount of opposition to bring out his energies and stimulate him to exertion, and though we are constantly talking in our country of the blessings of peace and horrors of war, we must generally acknowledge that our present foremost place among nations is due in a great degree to the keeping up of our innate energies by incessant turmoils and differences of opinion within and little wars and commercial rivalry without. It is not, then, to a reign of peace in which our energies would stagnate and become effete, but to a continuance of political excitement, which keeps the people on the alert, that we should be indebted for progress, and our statesmen should be sufficiently well educated and trained to take advantage of every time of excitement in furthering the welfare of the empire.

We owe the benefit (before railways) in the improvement of our Great Northern Roads for military purposes to the rebellion of 1745, leading to our being able to run coaches between London and Manchester in 1754, and between London and Edinburgh in 1763. Scotland and Ireland are both indebted to war and disorder for the first roads, constructed for purely military purposes.



But while the duty of taking advantage of each fitting opportunity for developing a country lies with the statesman, his prospect of success depends in great measure upon his geographical knowledge. His work may serve but for the purposes of the moment, and never benefit posterity, if he has no knowledge or foresight, no originality of purpose and perception of the fitness of things.

The measures that can be taken may be divided into two classes—domestic and international. The former designed to benefit the country or empire directly; the latter to shield the land from hostilities from without, and in which the consideration of geographical position has a most all-important bearing. In this latter class a complete knowledge of geography is an absolute necessity, as the question arises so often as to whether the acquisition of geographical positions will weaken or strengthen a kingdom. For example, were Ireland two degrees further to the west, it is probable that all our views as to the method of connecting it for administrative purposes with Great Britain would be greatly modified. Again, the particular points at which our coaling stations may be situated about the world may depend upon a variety of circumstances, changing from year to year. Thus Gibraltar, from its geographical position, was an absolute necessity to us thirty years ago, but, owing to various changes, it is not now of equal value, either as a coaling station, for protecting our commerce, or as a *dépôt* for our wares, and the question is arising with some geographers whether it might not with advantage be exchanged for Ceuta on the opposite coast.

It is possible that a more full geographical knowledge of Egypt and the Suez Canal might have materially modified our present occupation of Egypt. The canal could not be held without a fresh-water supply, and the possession of Cairo and the Nile is the key to the fresh-water canal supplying Ismailia and Suez. Had it been known that a plentiful supply of water could be obtained close to the marine canal, independent of the Nile water, it is questionable how far any occupation of Egypt would have been necessary.

In such cases it is not sufficient that the Government subordinates should have a knowledge of geography, for even if they are fully conversant with what they ought to know it would be almost impracticable for them to convey to statesmen knowledge which their untrained minds render them incapable of retaining or making use of.

In settling political boundaries it may appear at first sight that they should coincide with certain geographical features, forming natural boundaries not only in international matters, but also in cases of provincial, county, town, and parish boundaries, and also in accordance with historical associations; but we must do our statesmen the justice to admit that the deviations they adopt may not always be the result of ignorance, but arise from an astute perception that it may be necessary in the future to have a cause for further modification, or even for raising the whole question anew. It is difficult, however, to see how this can with any propriety arise in domestic matters, and, apart from the doubtful political morality involved, it would only occur in international matters on the assumption that our empire is paramount, and can quarrel when it chooses; and, moreover, in such a case could only be justified by being carried out with so perfect a knowledge of geography that in any reopening of the question our country should be in the right; whereas bitter experience has shown us that our statesmen have almost invariably placed us in the wrong.

It is fatal in domestic matters to ignore the physical features within a country, and attempt to obliterate its historical and topographical associations, as the French Revolutionists attempted, by substituting their departments for the old provinces. This has only led to an artificial division, which has not taken root among the people, and French geographers are still calling attention to the absurdity of present divisions. In such cases, we must keep alive to what are the ostensible and what the actual reasons for such changes, and if the so-called simplicity introduced by lawyer statesmen leads to increased law expenses, we may reasonably look with suspicion on such an interference with the economical administration of the affairs of the nation. In our own country geography is intimately connected with all kinds of divisions of land, which are dealt with by the administration. A simplification

of the arbitrary political divisions, and a modification and synchronisation of boundaries may lead directly to simplification of administrative machinery, and saving of expenses in salaries, &c. London itself is a glaring instance of the waste of money and friction of departments, from the extraordinary overlapping of boundaries—political, magisterial, petty-sessional, police, statistical, postal, public works, &c. Probably a great portion of the time and energies of the superior officers in the various departments is occupied in waging war on one another, keeping the peace, or temporising with or watching each other; and this not from their own desire to quarrel, but from the fault of the system which overlaps duties as well as boundaries, and often gives one and the same duties to be performed by distinct departments. Perhaps, in some instances, this friction may call out latent energy, but it at least most successfully prevents departmental superiors from looking into their own departmental affairs, and developing and perfecting the local administration, and keeping up to the times.

With regard to international boundaries, too little attention is usually paid to the changes which are caused by the advance of civilisation. For example, a natural boundary may, in time, become merely conventional owing to development of communications.

At one time the Rhine was a natural boundary, but it has now become a channel of communication. Again, the Zambesi is at present a natural boundary, completely separating distinct tribes; the time may come when it also will be a great channel of communication. The usual natural international boundaries are broad or rapid rivers and arms of the sea, mountain ranges, deserts, and swamps; but the highlands and lowlands of a country are also naturally separated, as they usually are inhabited by people of different nationality.

In Europe we find natural boundaries gradually losing their efficiency as political boundaries. The Rhine, for example, throughout a great portion of its length has ceased altogether to be a political boundary, for though it is still a military line of great strength, each large town on either bank has its suburb on the opposite side, and the population has become so assimilated that the river has ceased to be a practical political line. Consequently the line of the Vosges is deemed by many to have become the natural boundary between France and Germany, on account of its coinciding with the linguistic barrier. But, again, linguistic boundaries are no tests of the limits of nationalities or national feeling. When a foreign language is forced upon an unwilling people, they may for many generations be acutely opposed to the nation whose language they have adopted. On the Lower Danube, however, the physical, linguistic, and political divisions all coincide, and the river has become neutralised and is a natural boundary.

In central Europe we find the highlands of the Alps forming the natural and political boundary, though the people speak three different languages; but in these cases the people probably will not be found to be of the same race as those speaking the same language in the plains below.

Again, in the Pyrenees we find a natural, political, and linguistic barrier coinciding, assisted by the fact that the mountain people are a different race from those in the plains to the north and south.

In our own country we have a curious instance of language being no proof of the nationality of the people, as the Iberians in Wales speak Celtic, and the Celts in western Britain speak Anglo-Saxon. Again, in South Africa we have the people of French extraction speaking Dutch and still feeling resentment to the government on account of having forced a foreign language upon them, although the British have succeeded the Dutch.

Among Asiatic and African territories boundaries are often very ill-defined and uncertain. Frequently it happens that between two powerful states there is a large tract of country which owes a double allegiance, paying tribute to each, and yet in some respects remaining independent, probably consisting of lands which are easily ravaged and are comparatively speaking unprotected by nature.

When we look into the subject of boundaries among pastoral tribes, we find curious anomalies. The land belongs in many instances to the tribe and not to the individual, and cannot be alienated. In the desert of Arabia a tribe in one part will



have an interest in the date palms or corn lands of a tribe in another part, and this system is rather fostered than discountenanced, so that when evil befalls an individual in one part he may go and live with his tribal friends elsewhere. It is a knowledge of the intricate connections of these tribes and the topographic divisions of their lands which admits of any control being kept over these warlike people. A mistake arising out of a misunderstanding of this Bedouin system nearly led to a disastrous result in the Egyptian campaign of 1882, owing to an outlying branch of one of the most powerful tribes in Arabia being supposed to be a petty independent tribe of no consequence.

In many instances the cattle posts of tribes during peace time by mutual consent intermingle and overlap, yet are kept separate and distinct, so that no geographical boundary is practicable; in fact among such people it is the tribe before the territory which is under the control of the chief. Thus it is quite practicable to conceive instances of a tribe living on lands within the area occupied by another tribe and yet governed by its own laws. Many of the difficulties the British have encountered in South Africa have arisen from a complete ignorance of, or wilfully ignoring, the native land laws. Under the tribal system even the chiefs in council have not the power of disposing of any portion of the land they use; it belongs to every individual of the tribe and of the tribal branches and to their children's children. Thus, when a chief gives over his territory it does not follow that he gives over the land for disposal as crown lands, but only the government of the people. It is on this account that the offer of Khama and other chiefs of the Bechuanaland territory was of so great value. They proposed by agreement in council in their respective territories to hand over to Great Britain their territories, keeping for themselves the lands they used, and offering for emigration purposes their vast extents of hunting lands, which are not now of the same value for hunting purposes as they were in former days.

But this proposal has not been accepted, and a parallel of latitude has been proclaimed without consent of the Bechuana chiefs as the northern limit of the British Protectorate, cutting Khama's territory into two parts and cutting a portion of Matabeleland off from Lobongolo's territory; so that the Boers of the Transvaal cannot raid upon the Matabeles without violating the British Protectorate and *vice versa*, while we have no means of securing its protection. Again the Matabeles when making their annual raid upon Lake Ngami will violate the portion of the State of Khama without the Protectorate, and he, if he wishes to oppose them, must do so from his capital within the Protectorate. This will bring us into conflict with the Matabeles or else will practically deprive Khama of part of his territory.

It is difficult to conceive any arrangement more likely to lead to complications in the future. The Protectorate, based on geographical principles, should extend as far as the Zambesi, taking in all Khama's certain territory and as much of the neutral territory as might be necessary to provide a natural boundary to east and west.

In East Africa, again, the definition of spheres of action recently is anomalous. A boundary ten miles from the coast for the Zanzibar dominions can of course have only a tentative character, and the exact definition in the future cannot fail to lead to conflicts. Far worse, however, is the adoption of the river Tana as the northern boundary of the British sphere of influence—a river occupied on both banks by the same agricultural tribes. It is not clear for what reason the commissioners have left this difficulty for the future.

It would not be difficult to give many recent instances in which those charged with diplomatic definitions of international boundaries have failed in their duty owing to a want of geographical knowledge of the localities with which they had to deal.

For example the boundary treaty of 1783 with the United States was incapable of being carried into effect, as the geographical features did not correspond with the assumption of the commissioners. This led to a dispute lasting thirty years, resulting in the boundary treaty of August 9, 1843. The ignorance of the

geography of the country in this case led to very inconvenient and even disastrous results.

Again with the San Juan controversy. Historical and geographical knowledge and ordinary care for the future development of Canada might have led to such measures having been taken in the first instance as would have prevented cession of valuable positions to the United States in 1846.

In India, again, our want of knowledge of the country to the north of the Afghan boundary has led to a series of unnecessary concessions to Russia. Had the slightest encouragement been given in former years by the Indian Government to enable officers to acquire information as to the territories beyond our Indian Empire, no doubt we should now be in a more secure position.

But, fortunately for the British Empire, foreign politicians have also much to answer for to their respective countries on account of their ignorance of geography.

For many years past Germany has been increasing the population of the United States and our own colonies without assisting to further the influence of the German Empire; whereas had her statesmen been able to look forward, a German colony might have been established. Many Germans as far back as 1866 were desirous of establishing a colony in the Transvaal. But Germany now has to cast about for unoccupied territory, and has chosen a piece of useless territory on the western coast of South Africa, whereas with a little foresight Prince Bismarck might have obtained on easy terms the whole of the French colonies in the Gulf of Guinea and north of the Congo, which France had actually abandoned as worthless. Germany would thus probably have held the position of France with reference to the reversion of the Congo State.

By the treaty of Frankfort it was intended that all German-speaking villages were to be ceded to Germany, but the boundary as originally laid down, for want of geographical knowledge on the part of German employés, left several German villages near Metz in possession of France, and it was necessary subsequently to rectify the error.

As a section of the British Association we are interested in the development of geographical knowledge in the world generally, but more particularly in our own Empire, and it is only by unceasingly calling attention to our shortcomings with regard to the science which causes us to meet here to-day that we may hope for that progress to be made which will enable us to maintain the proud position we at present hold among nations owing to our practical skill and energy. Hitherto we have possessed so many other advantages that we have been able to dispense with a good system of instruction, but owing to various causes other nations are gaining upon us in various ways, and we in our turn should use every effort to successfully grapple with a subject which if properly taught must affect our welfare as a nation so deeply.



MANCHESTER, 1887.

## ADDRESS

TO THE

ECONOMIC SCIENCE AND STATISTICS SECTION

OF THE

BRITISH ASSOCIATION,

BY

ROBERT GIFFEN, LL.D., V.P.S.S.

PRESIDENT OF THE SECTION.

*The Recent Rate of Material Progress in England.*

IN coming before you on this occasion it has occurred to me that a suitable topic in the commercial capital of England, and at a time when there are many reasons for looking around us and taking stock of what is going on in the industrial world, will be whether there has been in recent years a change in the rate of material progress in the country as compared with the period just before. Some such question is constantly being put by individuals with regard to their own business. It is often put in political discussions as regards the country generally, with some vague idea among politicians that prosperity and adversity, good harvests and bad, in the most general sense, depend on politics. And it must always be of perennial interest. Of late years it has become specially interesting, and it still is so, because many contend that not only are we not progressing, but that we are absolutely going back in the world, while there are evident signs that it is not so easy to read in the usual statistics the evidence of undoubted growth as it was just before 1870-73. The general idea, in my mind, I have to add, is not quite new. I gave a hint of it in Staffordshire last winter, and privately I have done something to propagate it so as to lead people to think on what is really a most important subject. What I propose now to do is to discuss the topic formally and fully, and claim the widest attention for it that I possibly can.

There is much *primâ facie* evidence, then, to begin with, that the rate of the accumulation of wealth and the rate of increase of material prosperity may not have been so great of late years, say during the last ten years, as in the twenty or thirty years just before that. Our fair-trade friends have all along made a tactical mistake in their arguments. What they have attempted to prove is that England lately has not been prosperous at all, that we have been going backwards instead of advancing, and so on; statements which the simplest appeal to statistics was sufficient to disprove. But if they had been more moderate in their contentions, and limited themselves to showing that the rate of advance, though there was still advance, was different from and less than what it was, I for one should have been prepared to admit that there was a good deal of statistical evidence which seemed to point to that conclusion, as soon as a sufficient interval had elapsed to show that the

statistics themselves could not be misinterpreted. There has now been ample time to allow for minor variations and fluctuations, and the statistics can be fairly construed.

I have to begin by introducing a short table dealing with some of the principal statistical facts which are usually appealed to as signs of general progress and the reverse, and I propose to go over briefly the items in that table and to discuss along with them a few broad and notorious facts which cannot conveniently be put in the same form.

*Statement as to production or consumption of staple articles in the United Kingdom in the undermentioned years, with the rate of increase in different periods compared.*

|                                                            | 1855 | 1865              | 1875              | 1885                  | Ratio of increase % |         |         |
|------------------------------------------------------------|------|-------------------|-------------------|-----------------------|---------------------|---------|---------|
|                                                            |      |                   |                   |                       | 1855-65             | 1865-75 | 1875-85 |
| Income Tax assessments, mlln. £                            | 308  | 396               | 571               | 631                   | 28                  | 44      | 10      |
| Production of coal, million tons                           | 64   | 98                | 132               | 159                   | 53                  | 35      | 20      |
| "    pig iron    "                                         | 3.2  | 4.8               | 6.4               | 7.4                   | 50                  | 33      | 16      |
| Receipts from railway goods traffic per head of population | —    | 11s. <sup>1</sup> | 18s. <sup>1</sup> | 21s. 2d. <sup>1</sup> | —                   | 63      | 18      |
| Clearances of shipping in foreign trade, million tons      | 10   | 15                | 24                | 32                    | 50                  | 60      | 33      |
| Consumption of tea per head, lbs.                          | 2.3  | 3.3               | 4.4               | 5.0                   | 43                  | 33      | 13½     |
| "    sugar    "    "                                       | 30.6 | 39.8              | 62.7              | 74.3                  | 30                  | 58      | 19      |

The first figures are those of the income tax assessments. What we find is that if we go back thirty years and compare the amount of income tax assessments in the United Kingdom at ten years' intervals, there appears to be an immense progress from 1855 to 1875, the first twenty years, and since 1875 a much less progress. The total amount of the assessments themselves, stated in millions, was as follows:—

|              | Millions |              | Millions |
|--------------|----------|--------------|----------|
| 1855 . . . . | £308     | 1875 . . . . | £571     |
| 1865 . . . . | 396      | 1885 . . . . | 631      |

And the rate of growth in the ten yearly periods which these figures show is—between 1855 and 1865, 28 per cent.; between 1865 and 1875, 44 per cent.; and between 1875 and 1885, 10 per cent. only.

Making all allowance for changes in the mode of assessment by which the lower limit of the tax has been raised, for the apparent increase before 1875 which may have been due to a gradual increase of the severity of the collection, and for the like disturbing influences, I believe there is no doubt that these income tax assessments correspond fairly well to the change in the money value of income and property in the interval. How great the change in the rate of increase is, is shown by the simple consideration that if the rate of increase in the last ten years, instead of being 10 per cent. only, had been 44 per cent., as in the ten years just before, the total of the income tax assessments in 1885, which is actually 631 millions, would have been 882 millions! Something then has clearly happened in the interval to change the rate of increase.

These figures being those of money values, an obvious explanation is suggested which would account in great part for the phenomenon of a diminished rate of increase in such values without supposing a reduction of the rate of increase of real wealth, of the things represented by the money values, to correspond. This is the fall of prices of which we have heard so much of late years, and about which in some form or another we shall no doubt hear something at our present meeting. It is quite clear that if prices fall then income tax assessments must also be affected. The produce of a given area of land, for instance, sells for less than it would otherwise sell; there

<sup>1</sup> These figures are for 1860-64, 1870-74, and 1880-84.



is less gross produce, and in proportion there is even less net produce, that is, less rent ; consequently the net income appearing in the Income Tax Schedules is either less than it was or does not increase as it did before. The same with mines, with railways, and with all sorts of business under Schedule D. The things themselves may increase as they did before, but as the money values do not increase but diminish, the income tax assessments cannot swell at the former rate. It is the same with salaries and other incomes not dependent so directly in appearance on the fall in prices. Salaries and incomes are of course related to a given range of prices of commodities, and a fall in the prices of commodities implies that the range of salaries and incomes is itself lower than it would otherwise be, assuming the real relation between the commodities and incomes to be the same after the fall in prices as it would have been if there had been no fall in prices. Hence the income tax assessments by themselves are not a perfectly good test in a question like the present. The change implied may be nominal only, so far as the aggregate wealth and prosperity of the community are concerned, though of course there can be no great and general fall of prices without a considerable redistribution of wealth, which must have many important consequences.

This criticism, however, does not apply to the remaining figures in the short table submitted, and to various other well-known facts, which we shall now proceed to discuss.

The production of coal, then, is found to have progressed in the last thirty years as the income tax assessments have done. The figures in millions of tons at ten years' intervals are as follows :—

| Million Tons |    |              |     | Million Tons |  |  |  |
|--------------|----|--------------|-----|--------------|--|--|--|
| 1855 . . . . | 64 | 1875 . . . . | 132 |              |  |  |  |
| 1865 . . . . | 98 | 1885 . . . . | 159 |              |  |  |  |

And the rate of growth in the ten yearly periods which these figures show is between 1855 and 1865, 53 per cent.; between 1865 and 1875, 35 per cent.; and between 1875 and 1885, 20 per cent. only. The rate of growth in the last ten years is much less than in the twenty years just before. The percentages here, it will be observed, are higher than in the case of the income tax assessments. The increase in the last ten years in particular is 20 per cent. as compared with an increase of 10 per cent. only in the income tax assessments. But the direction of the movement is in both cases the same.

I need hardly say, moreover, that coal production has usually been considered a good test of general prosperity. Coal is specially an instrumental article, the fuel of the machines by which our production is carried on. Whatever the explanation may be, we have now, therefore, to take account of the fact that the rate of increase of the production of coal has been less in the last ten years than in the twenty years just before.

Then with regard to pig-iron, which is also an instrumental article, the raw material of that iron which goes to the making of the machines of industry, the table shows the following particulars of production :—

| Million Tons |     |              |     | Million Tons |  |  |  |
|--------------|-----|--------------|-----|--------------|--|--|--|
| 1855 . . . . | 3.2 | 1875 . . . . | 6.4 |              |  |  |  |
| 1865 . . . . | 4.8 | 1885 . . . . | 7.4 |              |  |  |  |

And the rate of growth which these figures show is between 1855 and 1865, 50 per cent.; between 1865 and 1875, 33 per cent.; and between 1875 and 1885, 16 per cent. only. Whatever the explanation may be, we have thus to take account of a diminution of the rate of increase in the production of pig-iron, much resembling the diminution in the rate of increase of the production of coal.

At the same time the miscellaneous mineral production of the United Kingdom has mostly diminished absolutely. On this head, not to weary you with figures, I have not thought it necessary to insert anything in the above short table; but I may refer you to the tables put in by the Board of Trade before the Royal Commission on Trade Depression. Let me only state very briefly that while the average annual amount of copper produced from British ores amounted in 1855 to

over 20,000 tons, in 1865 the amount was about 12,000 tons only, in 1875 under 5,000 tons, and in 1885 under 3,000 tons. As regards lead, again, while the production about 1855 was 65,000 tons, and in 1865 about 67,000 tons, the amount in 1875 had been reduced to 58,000 tons, and in 1885 to less than 40,000 tons. In white tin there is an improvement up to 1865, but no improvement since, and the only set-off, a very partial one, is in zinc, which rises steadily from about 3,500 tons in 1858, the earliest date for which particulars are given, to about 10,000 tons in 1885, considerably higher figures having been touched in 1881-83. There is nothing, then, in these figures as to miscellaneous mineral production to mitigate the impression of the diminution in the rate of increase in the great staples, iron and coal, in recent years.

Agricultural production, it is also notorious, has been at any rate no better, or not much better, than stationary for some years past, although down to a comparatively recent period a steady improvement seemed to be going on. Making all allowance for the change in the character of the cultivation, by which the gross produce is diminished, although the net profit is not affected to the same extent, and which might be held to argue no real decline in the rate of general growth if the population, diverted from agriculture, were more profitably employed, yet the facts, broadly looked at, taken in connection with the other facts stated as to diminished rate of increase in other leading industries, seem to confirm the supposition that there may have been some diminution in the rate of increase generally.

It is, unfortunately, impossible to state in a simple manner the progress at different dates in the great textile industries of the country. Everything as regards these industries is thrown out by the disturbance consequent on the American War. It does not appear, however, that what has happened as regards the main textile industries, cotton and wool, would alter sensibly the conclusions above stated, drawn from the facts as to other main industries of the country. If we take the consumption of raw materials as the test, it would appear that the growth in the cotton manufacture is from a consumption of 28 lbs. per head in 1855 to about 38 lbs. per head in 1875, while in 1885 the consumption is nearly 42 lbs. per head, an increase of 4 lbs. per head in the last ten years, against 10 lbs. per head in the previous twenty. The percentage of increase in the last twenty years must therefore, on the whole, have been less than in the previous twenty, although in these twenty years the great interruption due to the American Civil War occurred. Of course the amount of raw material consumed is not here an absolute test. There may be more spinning and weaving now in proportion to the same quantity of raw material than was formerly the case. But the indications are at least not so certain and direct as when the consumption of raw material could be confidently appealed to. As regards wool the comparison is unfortunately very incomplete owing to the defect of data for the earlier years; but what we find is that the amount of wool consumed per head of the population of the United Kingdom has in the last ten years rather declined than otherwise from nearly 11 lbs. per head in the five years 1870-74 to 10 lbs. per head only in the five years 1880-84. Here, again, the explanation suggested as to cotton—viz. that there may be more spinning and weaving now in proportion to the same quantity of raw material than was formerly the case—applies. But the answer is also the same, that at any rate the indications of progress are no longer as simple as they were. The reality of the former rate of advance is not so clearly manifest.

Of course I need hardly add that in the case of another great textile, silk, there has been no progress, but the reverse, for some years; that this is also true of linen; and that the increase in the allied manufacture, jute, can only be a partial set-off.

In the textiles, then, as in other staple industries of the country, the rate of advance in the last ten years, measuring by things, and not merely by values, has been less than in the twenty years immediately before.

We pass on, then, to another set of figures included in the short table above submitted. We may look not only at leading industries of production directly, but at the broad figures of certain industries which are usually held to reflect, as in a mirror, the progress of the country generally. I refer to the railway traffics as



regards the home industries of the country, and the entries and clearances of shipping in the foreign trade as regards our foreign business.

As regards railways what we find is, if we take the receipts from the goods traffic in the form in which they were summarised for the Royal Commission on Trade Depression, viz., reduced to so much per head of the population on the average of quinquennial periods, that in the five years 1860-64, which is as far back as the figures can be carried, the receipts per head were 11s.; ten years later, viz. in 1870-74, the receipts per head were 18s.; and ten years later, viz. 1880-84, the receipts per head were 21s. 2d. The rate of growth shown in the first ten years' interval is 63 per cent.; in the second ten years' interval it is only 18 per cent.; and in the last year or two, I may add, there has been no further improvement. Here the question of the value of money comes in again, but this would only modify partially the apparent change. There is also a question as to railway extension having been greater in the earlier than in the later period, so that growth took place in the earlier period because there were railways in many districts where they had not been before, and there was no room for a similar expansion in the later period. But the difference in the rate of growth it will be observed is very great indeed, and this explanation seems hardly adequate to account for all the difference. At any rate, to repeat a remark already made, the indications are no longer so simple as they were. There is something to be explained.

The figures as to the number of tons of goods carried are not in the above table; nor are such figures very good, so long as they are not reduced to show the number of tons conveyed one mile. But, *quantum valeant*, they may be quoted from the Board of Trade tables already referred to. The increase, then, in minerals conveyed between 1855 and 1865 is from about 40 million to nearly 80 million tons, or 100 per cent.; between 1865 and 1875 it is from 80 to about 140 million tons, or 75 per cent.; and in the last ten years it is from 140 to 190 million tons only, if quite so much, or about 36 per cent. only. As regards general merchandise, again, the progression in the three ten yearly periods is in the first from about 24 to 37 million tons, or rather more than 50 per cent.; in the second from 37 to 63 million tons, or 70 per cent.; and in the third from 63 to 73 million tons, or 16 per cent. only. As far as they go there is certainly nothing in these figures to oppose the indications of a falling-off in the rate of increase of general business already cited.

Coming to the movement of shipping in the foreign trade the series of figures we obtain are the following, which relate to clearances only, those relating to entries being of course little more than duplicate, so that they need not be repeated: 1855, 10 million tons; 1865, 15 million tons; 1875, 24 million tons; 1885, 32 million tons. And the rate of growth thus shown is between 1855 and 1865 no less than 50 per cent.; between 1865 and 1875 no less than 60 per cent.; and between 1875 and 1885 about 33 per cent. only—again a less rate of increase in the last ten years than in the period just before. Here, too, it is to be noticed, what is unusual in shipping industry, that in the last few years the entries and clearances in the foreign trade have been practically stationary. The explanation no doubt is in part the great multiplication of lines of steamers up to a comparatively recent period, causing a remarkable growth of the movement while the multiplication of lines was itself in progress, and leaving room for less growth afterwards because a new framework had been provided within which traffic could grow. But here again it is to be remarked that the whole change can hardly, perhaps, be explained in this manner, while the remark already made again applies, that the fact of explanation being required is itself significant.

The figures of imports and exports might be treated in a similar manner, as they necessarily follow the course of the leading articles of production and the movements of shipping. But we should only by so doing get the figures we have been dealing with in another form, and repetition is of course to be avoided.

The short table contains only another set of figures, viz. those of the consumption of tea and sugar, which are again commonly appealed to as significant of general material progress. What we find as regards tea is that the consumption per head rises between 1855 and 1865 from 2·3 to 3·3 lbs., or 43 per cent.; between 1865 and



1875 from 3·3 to 4·4 lbs., or 33 per cent.; and between 1875 and 1885 from 4·4 to 5 lbs., or 13½ per cent. In sugar the progression is in the first period from 30·6 to 39·8 lbs. per head, or 30 per cent.; in the second period from 39·8 to 62·7 lbs., or 58 per cent.; and in the third period from 62·7 to 74·3 lbs., or 19 per cent. only. In the last ten years in both cases the rate of increase is less than in the twenty years before.

These facts, I need hardly say, would be strengthened by a reference to the consumption of spirits and beer, the decline in the former being especially notorious. In tobacco again in the last ten years there has been no increase of the consumption per head; which contrasts with a rapid increase in the period just before—viz., from about 1·31 lb. per head in 1865 to 1·46 lb. per head in 1875.

No doubt the observation here applies that the utmost prosperity would obviously be consistent with a slower rate of increase per head from period to period in the consumption of these articles, and with, in the end, a cessation of the rate of increase altogether. The consumption of some articles may attain a comparatively stationary state, the increased resources of the community being devoted to new articles. But here, again, we have to observe the necessity for explanation. The indications are no longer so sure and obvious in all directions as they were.

It is difficult, indeed, to resist the impression made when we put all the facts together, leaving out of sight for a moment those of values only. We are able to affirm positively—(a) That the production of coal, iron, and other staple articles has been at a less rate in the last ten years than formerly; (b) that this has taken place when agricultural production has been notoriously stationary, and when the production of other articles such as copper, lead, &c., has positively diminished; (c) that there has been a similar falling-off in the rate of advance in the great textile industries; (d) that the receipts from railway traffic and the figures of shipping in the foreign trade show a corresponding slackening in the rate of increase in the business movement; and (e) that the figures as to consumption of leading articles, such as tea, sugar, spirits, and tobacco, in showing a similar decline in the rate of increase, and, in some cases, a diminution, are at least not in contradiction with the other facts stated, although it may be allowed that there was no antecedent reason to expect an indefinite continuance of a former rate of increase.

From these facts, however, we may qualify them: and many qualifications have already been suggested while others could be added: it seems tolerably safe to draw the conclusion that there has probably been a falling-off in the rate of material increase generally. The income tax assessment figures, though they could not be taken by themselves in such a question, are, at least, not in contradiction, and there is nothing the other way when we deal with these main figures only. I should not put the conclusion, however, as more than highly probable. Some general explanation of the facts may be possible on the hypothesis that there is no real decline in the rate of growth generally at all; that the usual signs for various reasons have become more difficult to read; that owing to the advance already made the real growth of the country and, to some extent, of other countries, has taken a new direction; and that the utmost caution must be used in forming final conclusions on the subject. But the conclusion of a check having occurred to the former rate of growth may be assumed meanwhile for the purposes of discussion. The attempted explanation of the causes of change, on the hypothesis that there is a real change, may help to throw light on the question of the reality of the change itself.

Various explanations are suggested, then, not only for a decline in the rate of our progress, but for actual retrogression. Let us look at the principal of these explanations in their order and see whether they can account for the facts: either for actual retrogression, or for a decline in the general rate of material growth equal to what some of the particular facts above cited, if they were significant of a general change in the rate of growth, imply—a decline, say, from a rate of growth amounting to 40 per cent. in ten years to one of 20 per cent. only in the same period.

One of the most common explanations, then, as we all know, is foreign competi-



tion. The explanation has been discredited because of the exaggeration of the alleged evil to be explained; but it may possibly be a good enough explanation of the actual facts when they are looked at in a proper way. In this light, then, the assertion as to foreign competition would be found to mean that foreigners are taking away from us some business we should otherwise have had, and that, consequently, although our business on the whole increases from year to year, it does not increase so fast as when foreign competition was less. Those who talk most about foreign competition have actually in their mind the unfair element in that competition, the stimulus which the Governments of some foreign countries give or attempt to give to particular industries by means on the one hand of high tariffs keeping out the goods we should otherwise send to such countries, and giving their home industry of the same kind a monopoly which sometimes enables them to produce a surplus they can sell ruinously cheap abroad, and, by means on the other hand of direct bounties which enable certain industries to compete in the home market of the United Kingdom itself as well as in foreign markets. But there is a natural foreign competition as well as a stimulated foreign competition to be considered, and it may be the more formidable of the two.

Dealing first with the stimulated competition, the most obvious criticism on this alleged explanation of the recent decline in the rate of increase of our material progress is that the stimulus given by foreign Governments in recent years has not been increasing, or, at any rate, not materially increasing, so as to account for the change in question. People forget very quickly; otherwise it would not be lost sight of that after 1860, as far as European nations are concerned, there was a great reduction of tariff duties—a change, therefore, in the contrary direction to that stimulus which is alleged to have lately caused a change in the rate of our own development. Since about five or six years ago the movement on the Continent seems again to have been in the direction of higher tariffs. France, Italy, Austria, Germany, and Russia have all shown protectionist leanings of a more or less pronounced kind. Some of our colonies, especially Canada, have moved in the same direction. But, on the whole, these causes as yet have been too newly in operation to affect our industry on a large scale. As a matter of fact, with one exception to be presently noticed, the period from 1860 to 1880 was one in which the effect of the operation of foreign Governments in regard to their tariffs could not be to stimulate additional competition of an injurious kind with us in the way above described, but to take away, if anything, from the stimulus previously given. The changes quite lately brought into operation, if big enough, and if really having the effects supposed, might stimulate foreign competition in the way described in the period now commencing; but, as an explanation of the past facts, it is impossible to urge that foreign competition had recently been more stimulated by additions to tariffs than before, and that in consequence of this stimulus our own rate of advance had been checked.

The one exception to notice is the United States. Immediately after 1860 the civil war in that country broke out, and that war brought with it the adoption of a very high tariff. Curiously enough, however, that tariff operated most against us in the very years, that is, the years before 1875, in which our rate of advance was greater to all appearance than it has lately been. In 1883 there was a great revision of the tariff, having for its general result a slight lowering and not an enhancement of the tariff, and it is with this reduction, that is, with a diminution of the alleged adverse stimulus, that the diminution in our own rate of advance has occurred.

Of course the explanation may be that, although Governments have not themselves been active till quite lately in adding to their tariffs, yet circumstances have occurred to make the former tariffs more injurious in recent years than they were down to 1875. For instance, it may be said that owing to the fall of prices in recent years the burden of specific duties has become higher than it was. The duty is nominally unchanged, but by the fall of prices its proportion to the value of the article has become higher. This is no doubt the case to a large extent. On the other hand *ad valorem* duties have been lowered in precisely the same way. The fall of prices has brought with it a reduction of duty, and especially on articles of



English manufacture, where the raw material is obtained from abroad, the reduction of duty, being applicable to the whole price, must certainly have had for effect to render more effective than before the competition of the English manufacturer. Whether on the whole the reduction of *ad valorem* duties consequent on the fall of prices has been sufficient throughout the range of our foreign trade to compensate the virtual increase of the weight of specific duties from the same cause seems to be a nice question. This being the case, it must be very difficult indeed to show that on the whole the weight of foreign tariffs, apart from the action of foreign Governments, has been increased in recent years so as to affect our own growth injuriously.

Foreign tariffs, it may be said, have become more effective for another reason. Manufacturing industry having itself developed abroad, the same amount of protection given to the foreign industry becomes more efficient than it was. But this, of course, raises the question of the effect of natural foreign competition, which will presently be discussed.

So much for the stimulus to foreign competition due to high tariffs. With regard to bounties very little need be said. They have been the subject of much discussion and agitation for various reasons, and in what I have to say I propose not to touch on the practical question whether these bounties are injurious, and the nature of the political remedies that may or may not be possible. I limit myself strictly to the point, how far any effect which such bounties can have had would account for a diminution in the rate of material growth of the country generally in the last ten years as compared with the ten years just before. Dealing with the question in this strictly limited fashion, what I have to observe first is that hitherto very few bounties have been complained of, except those on sugar production and refining; and next that the whole industries of sugar production and refining, important as they are in themselves, hardly count in a question of the general industry of the United Kingdom. Even if we refined all the sugar consumed in the United Kingdom and the maximum amount we have ever exported, the whole income from this source, the whole margin, would not exceed about 2,000,000*l.* annually, not one-six-hundredth part of the income of the people of the United Kingdom; and of this 2,000,000*l.* at the worst we only lose a portion by foreign competition, while all that is really lost, it must be remembered, is not the whole income which would have been gained if a certain portion of our labour and capital had been employed in sugar refining, but only the difference between that income and the income obtained by the employment of the same labour and capital in other directions. The loss to the empire may be greater because our colonies are concerned in sugar production to the extent at present prices of 5,000,000*l.* to 6,000,000*l.* annually, which would probably be somewhat larger but for foreign competition. But it does not seem at all certain that this figure would be increased if foreign bounties were taken away, while in any case the amounts involved are too small to raise any question of foreign bounties having checked the rate of growth of the general industry of the country.

*Per contra*, of course, the extra cheapness of sugar, alleged to be due to the bounties, must have been so great an advantage to the people of the United Kingdom, saving them perhaps 2,000,000*l.* to 3,000,000*l.* per annum, that the stimulus thereby given to other industries must apparently have far more than compensated any loss caused by the stimulus of foreign bounties to sugar production and refining abroad. But to enlarge on this point would involve the introduction of controversial matter, which I am anxious to avoid. I am content to show that nothing that can have resulted from sugar bounties could have affected seriously the general rate of material growth in the country.

*Mutatis mutandis*, the same remarks apply to other foreign bounties, of which indeed the only ones that have been at all heard of are those on shipping. But as yet, at least, the increase of foreign shipping has not been such as to come into comparison with our own increase, while the portion of the increase that can be connected with the operation of bounties is very small. It would be useless to

<sup>1</sup> See Appendix to *First Report of Royal Commission on Trade Depression*, p. 130.



enter into figures on so small a point; but few figures are so well known or accessible as those relating to shipping.

In neither way, then, does there appear to be anything in the assertion that the protectionist action of foreign Governments in recent years can have caused the check alleged to the rate of growth in our industry generally, assuming such a check to have occurred. I may be dispensed, therefore, from entering on the theoretical argument, which I only notice *pour mémoire*, that in the nature of things no enhancement of foreign tariffs and no grants of foreign bounties could really check our own rate of growth, except by checking foreign growth still more, which is not the case we are considering, because the allegation is that foreign competition is increasing at our expense. That I do not insist on this argument is not to be considered as a sign that it is dropped or that I am not fully sensible of its logical completeness. It seems enough, at present, to fortify it by considerations from actual practical facts which no one can dispute.

The question of an increase of foreign competition from natural causes is more difficult. It is beyond all question, as I have pointed out elsewhere, that foreign competition in every direction from natural causes must continue to increase, and that it has increased greatly in recent years. But when the facts are examined, it does not appear that this competition has been the cause of a check to our own rate of growth. One of the facts most commonly dwelt upon in this connection is the great increase of the imports of foreign manufactured articles into the United Kingdom. But the increase in the last ten years is not more than about 18,000,000*l.*, taking the facts as recorded in what is known as Mr. Ritchie's return, viz., from about 37,000,000*l.* in the quinquennial period 1870-74 to 55,000,000*l.* in the quinquennial period 1880-84, or about 50 per cent. Out of 18,000,000*l.* increased imports of such articles it is fair to allow that at least one-half, if not more, is the value of raw material which we should have had to import in any case; so that only 9,000,000*l.* represents the value of English labour displaced by these increased imports. Even the whole of this 9,000,000*l.* of course is not lost, only the difference between it and the sum which the capital and labour 'displaced' earns in some other employment, which may possibly even be a *plus* and not a *minus* difference. If we add articles 'partly manufactured' no difference would be made, for the increase here is only from 26,000,000*l.* to 28,000,000*l.* in the ten years. Such differences, it need not be said, hardly count in the general total of the industry of the country. Further, the rate of increase of these imports was just as great in the period when our own rate of growth was greater as in the last ten years, the increase in manufactured articles between 1860-64 and 1870-74 being 19,000,000*l.*, viz., from 18,000,000*l.* to 37,000,000*l.*, or over 100 per cent. as compared with 50 per cent. only in the last ten years, and in articles partly manufactured from 17,000,000*l.* to 26,000,000*l.*, an increase of 9,000,000*l.* as compared with an increase of 2,000,000*l.* only in the last ten years. Making all allowance for the fall in prices in recent years, these figures still show a greater relative increase of imports of manufactured articles before 1875 than afterwards. It cannot, therefore, be the increased import of foreign manufactures which has caused the check to our own growth in the last ten years.

But foreigners, it is said, exclude us from their own markets and compete with us in foreign markets. Here again, however, we find that any check which may have occurred to our foreign export trade is itself so small that its effect on the general growth of the country would be almost *nil*. Take it that the check is as great as the diminution in the rate of increase in the movements of shipping, viz., from an increase of about 55 per cent. to one of 33 per cent. only, that is, broadly speaking, a diminution of one-third in the rate of increase of our foreign trade, whatever that rate may have been. Assuming that rate to have been the same as the rate of increase in the movements of shipping itself, the change would be from a rate of increase equal to one-half in ten years to a rate of increase equal to about one-third only. Applying these proportions to the exports of British and Irish produce and manufactures, which represent the productive energy of the country devoted to working for foreign exchange, and assuming that ten years ago the value of British labour and industry in the produce and manufactures we exported, due deduction

being made for the raw material previously imported, was about 140,000,000*l.*,<sup>1</sup> then it would appear that if the same range of values had continued the check to the growth of this trade would have been that at the end of ten years the British labour represented in it instead of having increased 50 per cent., viz., from 140,000,000*l.* to 210,000,000*l.*, would have increased one-third only, or from 140,000,000*l.* to about 187,000,000*l.* The annual difference to the energy of the country developing itself in the foreign trade would on this showing be about 23,000,000*l.* only, an insignificant sum compared with the aggregate income of the people of the country; while the country, it must be remembered, does not lose the whole of this sum, but only the difference between it and the sum earned in those employments to which those concerned have resorted, which again may be a *plus* and not a *minus* difference. Even therefore if foreign competition is the cause of a check to our general growth, yet the figures we are dealing with in our foreign trade are such that any visible check to that trade which can have occurred must have been insufficient to cause that apparent diminution in the rate of our material growth generally which has to be explained.

It has to be remembered, moreover, that when the figures are studied and the fall of prices allowed for it is not in our foreign trade that any check worth mentioning seems to have occurred at all. The diminution in the rate of increase in the movements of shipping is very largely to be accounted for in the way already explained, viz., by the fact that the increase just before 1875 was largely owing to the multiplication of lines of steamers, and that a framework had then been provided up to which the traffic has since grown. Even an increase of one-third in the movements in the last ten years may thus show as great an increase in real business as an increase of 50 or 60 per cent. in the movements in the twenty years before. Foreign competition, even from natural causes, is thus insufficient to account for the diminution in the rate of increase of our material growth in the last ten years.

These figures may be put directly another way. The increase of our foreign exports per head between 1860-64 and 1870-74 was from 4*l.* 14*s.* 11*d.* to 7*l.* 7*s.* 5*d.*, or about 55 per cent., and allowing for an average rise of prices between the two dates may be put as having been at the extreme about 50 per cent. Between 1870-74 and 1880-84 instead of an increase there is a decrease, viz., from 7*l.* 7*s.* 5*d.* to 6*l.* 12*s.* 9*d.*, but deducting about one-third from the former figure for the fall in prices, the real increase in the last ten years would appear to be as from 4*l.* 16*s.* 3*d.* to 6*l.* 12*s.* 9*d.*, or over 35 per cent. The difference in the rate of increase in the last ten years compared with the previous ten is thus the difference between 35 and 50 per cent. only, equal to about 21,000,000*l.* annually on the amount of 140,000,000*l.*, assumed to represent the value of British industry in our foreign exports, deduction being made for the value of raw material included. A deduction of this sort from the annual income of the country is too small to account for such a check to the rate of our growth generally as that we are now discussing as probable, especially when we recollect that the labour is only diverted, and it is not the whole 21,000,000*l.* that is lost, but only the difference between that sum and what is otherwise earned, which may even be a *plus* and not a *minus* difference.

To bring the matter to a point, an increase of 40 per cent. in the income of the country in ten years would on an assumed income of 1,000 millions only in 1875, and the figure must then have been more, have brought the income up to 1,400 millions; an increase of 20 per cent. would have brought it up to 1,200 millions only, a difference of 200 millions, which must have arisen from the alleged difference in the rate of our material growth in question if it had occurred. Clearly nothing can have happened in our foreign trade to account for anything more than the smallest fraction of such a difference. The figures are altogether too small. We may repeat again then that it is not the check to our foreign trade which foreign competition may have caused to which we can ascribe the recent check to our general rate of growth.

I need hardly add that in point of theory foreign competition was not likely to

<sup>1</sup> See my *Essays in Finance*, 1st series, p.



have the effect stated. I have set forth elsewhere in an elaborate essay<sup>1</sup> the reasons for holding this opinion; why it is, in fact, that as foreign nations grow richer we should be better off absolutely than if they were to remain poor, though relatively they might advance more than we do. But, whatever theory may say, in point of fact the check to the rate of our material growth cannot, for the reasons stated, have been due to anything which has happened to our foreign trade.

Another explanation which has been suggested, and to which I have myself been inclined to attach considerable weight as being plainly, as far as it goes, a *vera causa*, is the extent to which the hours of labour have been reduced in many employments in consequence of the improvement in the condition of the working classes in the last half-century, and the growth of a disposition to take things easier, which has been the result of the general prosperity of the country. Such causes when they exist, and when they are brought into operation, must tend to diminish the rate of material growth in a country as compared with a period just before when they were not in operation. If we could suppose them brought into operation suddenly, all other things, such as the progress and development of invention, remaining the same, such a reduction of hours of labour and growth of a disposition to take things easy, must produce a check to the former rate of growth.

After some consideration, however, although there is no doubt of the general tendency of the causes referred to, I begin to doubt whether they would explain adequately such a check to the rate of material growth generally throughout the country as is assumed to have occurred. As regards the shortening of the hours of labour, which is the more definite fact to be dealt with, it cannot but be observed that the shortening has by no means been universal. It has been conspicuous among certain trades organised into trades unions; but the unions, after all, only include about a tenth part of the labour of the country. There has been no such conspicuous shortening of the hours of labour among professional men, clerks, domestic servants, and many others whose labour is an essential part of the general sum total. Next—and this is perhaps even more important—the shortening of the hours of labour is not coincident with the beginning of the last ten years, though it has been in full operation for the whole of that period, but rather with the beginning or middle of the previous ten years, viz., 1865–75; so that it should have been fully in operation upon the production of 1875; and the check to our rate of growth if due to this cause should thus have been felt between 1865 and 1875, rather than between the latter date and the present time. The same with the general disposition to take things easy. This disposition did not spring up in a day in 1875, but was probably as effective as a cause of change in the earlier, as in the later, period. It must count for something as a cause of the annual production of the country being less at a given moment than it would otherwise be; but in comparing two periods what we have to consider is whether the growth of this disposition has been greater in one period than in another; and there are no data to support such a conclusion as regards the last ten years compared with the previous ten.

We must apparently, therefore, reject this explanation also. It is not adequate to account for the apparent change that has occurred in the rate of our growth from the year 1875 as compared with the period just before. Our progress in periods previous to 1875 took place in spite of the operation of causes of a similar kind which were then in operation, and there is no proof at all that the shortening of the hours of labour and the growth of a disposition to take things easy have been greater since 1875 as compared with the period just before than they were between 1865 and 1875 as compared with the period just before that. What is wanted is a new cause beginning to operate in or about 1875, and the shortening of the hours of labour and the growth of a disposition to take things easy do not answer that description sufficiently. Something of the apparent change may be due to an acceleration in recent years of the growth of a disposition to take things easy, but on the whole the explanation halts when we make a strict comparison.

Another cause which may properly be assigned as a *vera causa* of a check to the

<sup>1</sup> See my *Essays in Finance*, 2nd series, 'Foreign Manufactures and English Trade.'

rate of material growth in the country is the unfavourable weather to agriculture and the generally unprofitable conditions of that industry in recent years. *Pro tanto* such influences would make agricultural production less to-day than it would otherwise be. Employment in that industry would also be diminished comparatively, and perhaps absolutely, and a check to production generally would take place while labour was seeking new fields. But the check arising in this manner, as far as the general growth is concerned, has obviously not been very great. More land in proportion has been turned into permanent pasture, but very little land has gone out of cultivation altogether, and even the amount under the plough has not much diminished. Agricultural labour, in somewhat greater proportion than before, has been obliged to seek other employments; the flow of population from country to town has been increased somewhat; but nothing new has happened to diminish production generally to a serious extent, and it is a new cause, it must be remembered, for which we are seeking. As far as unfavourable weather is concerned, again, that is only a temporary evil. One year with another, the weather is not worse now than at any former time; the remarkably unfavourable weather which lasted from 1874 to 1880 has passed. The other conditions unfavourable to agriculture, especially foreign competition, are more enduring; but these seem much more unfavourable to rent than to production itself, which is the point now under consideration; and we do not know that they will be permanent at all when prices and wages are fully adjusted.

The disturbance to industry by the fall of prices generally is also a *vera causa* of a check to the rate of material growth. But the effect of such a cause seems to be confined within narrow limits, and it is not a new cause. It occurs in every time of depression due to discredit, being partly the effect and partly the cause of the depression itself. All that is new recently is the extreme degree of the fall, and I must express the greatest doubt whether a mere difference of degree aggravates materially the periodical disturbance of industry, tending to check production, which a fall of prices from a high to a low level causes. So far as past experience has gone, at any rate, no such cause has been known to check production to any material extent. If any such cause tended to have a serious effect we should witness the results every time there is a shrinkage of values owing to the contraction and appreciation of an inconvertible paper currency, and I am not aware of any such contraction having had the effect described on production, though the effect in producing a feeling of depression is beyond all question. The facts as to the great contraction in this country between 1815 and 1820 are on record, while the experience of the United States after the civil war is also fresh in everyone's recollection. Contraction of currency and fall of prices, though they are painful things, do not stop production materially.

Another explanation suggested is that there is in fact no antecedent reason for supposing that the rate of material growth in a community should always be at the same rate—that a community may, as it were, get 'to the top' as regards its development under given conditions, and then its advance should be either less rapid than it had been or it should even become stationary. The defect of this explanation is that it assumes the very thing which would have to be proved. Is there any other sign except the alleged check to the rate of our material growth itself that in or about the year 1875 this country got 'to the top'? It has, moreover, to be considered that on *à priori* grounds it is most unlikely a community would get to the top *per saltum*, and then so great a change should occur as the apparent change we are considering. The persistence of internal conditions in a given mass of humanity is a thing we may safely assume, and if these conditions are consistent with a given rate of development in one period of ten years, it is most unlikely that, save for an alteration of external conditions, there would be another rate of development in the succeeding ten years. Human nature and capacities do not change like that. Scientific opinion, I believe, is also to the effect that the progress of invention and of the practical working of inventions, which have been the main cause of our material growth in the past, have been going on in the last ten years, are still going on, and are likely to go on in the near future, at as great a rate as at any time in the last fifty years. Except, as already said, the apparent



check to the rate of our material growth itself, there is no sign anywhere of our having got to the top, so that a stationary condition economically, or a condition nearly approaching it, has been reached.

Last of all, it is urged that the diminution in the material of our growth, which is in question, must be due to the fact that we are losing the natural advantages of coal and iron which we formerly had in comparison with the rest of the world. This is perhaps only another way of saying that we have got to the top by comparison, though the community of nations generally has not got to the top, and another way of saying also that foreign competition affects us more than it formerly did; an argument already dealt with. But the question whether coal and iron at home are really so indispensable to our material growth as is sometimes assumed appears itself so important that I may be excused for specially discussing this question, notwithstanding that it has virtually been disposed of, as far as any explanation of past facts is concerned, by what has been already said.

The argument proceeds on the supposition—which is no doubt well founded in the abstract and as far as the past experience of mankind is concerned—that in addition to natural capacities of its own a community requires for its prosperity certain natural advantages, fertility of soil, rich and easily worked mines, a genial climate in which labour may conveniently be carried on, and so forth. A community possessing all these things, or the like things, will flourish, but as it ceases to lose any of them its prosperity must become precarious, and population must flow to the places where they can be secured. Of course climate is not a thing which changes, as far as any practical experience is concerned; but relatively the advantage of a fertile soil may be lost, as England has lately lost it in comparison with the United States and other new countries, its soil having become inadequate for the whole population; and still more the advantage of mines, especially mines of coal and iron, on which the miscellaneous industries of a manufacturing country depend, may be lost. Hence it is said the check to our rate of growth in recent years. We have long since lost our agricultural advantages by comparison. Now we are also beginning to lose the special advantages which coal and iron have given. Our mines are becoming less rich than those of foreign countries, and the balance is turning against us. Why should not population relatively flow from England to the United States and other countries as it has passed within the limits of the United Kingdom itself from Cornwall and Sussex to Staffordshire, Lancashire, Yorkshire, and the north? In this view the coal famine of 1873 was the sign of a check such as Mr. Jevons anticipated. What has happened since is only a sequence of the like causes.

I need not repeat in opposition to this view what has already been said as to the inadequacy of any actual decline in our foreign trade to account for such a check to our general growth as is supposed to have occurred. If the loss of our natural advantages of coal and iron in addition to agriculture are having the effects supposed, we ought to witness them in our foreign trade, and in fact we do not witness them to the extent required for the production of the phenomenon in question.

What I wish now specially to urge is that in consequence of the progress of invention and the practical application of inventions in modern times the theory itself has begun to be less true generally than it has been. It is no longer so necessary, as it once was, as in fact it always has been until very lately, that people should live where their food and raw materials are grown. The industry of the world having become more and more manufacturing and, if one may say so, artistic, and less agricultural and extractive, the natural advantages of a fertile soil and rich mines are less important to a manufacturing community than they were at any former period of the world's history, because of the new cheapness of conveyance. Under the new conditions, I believe it is impossible to doubt, climate, accumulated wealth, acquired manufacturing skill, concentration of population become more important factors than mere juxtaposition to the natural advantages of fertile soil and rich mines. The facts seem at any rate worth investigating, judging by what has happened in England and other old countries in the last half-century and by what is still happening there.

Take first the question of food. Wheat is now conveyed from the American

Far West to Liverpool and London and any other ports in the Old World for something like five shillings per quarter—equal to about half a farthing on the pound of bread, or a halfpenny on the quartern loaf. The difference between the towns of a country with fertile soil, therefore, and the towns of a country with inadequate soil is represented by this small difference in the price of bread. At about fivepence the quartern loaf the staff of life may be about 10 per cent. cheaper in the fertile country than it is in a country which does not grow its own food at all, and which may be thousands of miles away. As the staff of life only enters into the expenditure of the artisan to the extent of 20 per cent. at the outside, and into the expenditure of richer classes to a smaller extent, the difference on the whole income of a community made by their living where the staff of life would be cheaper would be less than 2 per cent.—too small to tell against other advantages which may be credited to them. What is true of wheat is even more true of meat and other more valuable articles of food where the cost of conveyance makes a less difference in the proportionate value of the food *in situ* and its value at a distant point. The same more and more with raw materials. Cotton and such articles cost so little to transport that the manufacturing may as well go on in Lancashire or any other part of the Old World as *in situ* or nearly *in situ*; and even as regards metals or minerals, except coal and perhaps iron, the same rule applies, the cost of conveyance being as nothing in proportion to the value of the raw material itself. As regards coal and iron, moreover, there are many places where they are not in absolute juxtaposition, and if they have to be conveyed at all they may as well be conveyed to a common centre. Iron ore and iron at any rate are beginning to be articles of import into the old countries of Europe to which the cost, in fact, offers very little difficulty. The additional cost to the miscellaneous manufacturing of a country through its having to bring iron and coal from a distance may thus be quite inconsiderable, and apparently is becoming more and more inconsiderable. As regards raw materials generally it has also to be considered that, owing to their immense variety, there is an undoubted convenience in a common manufacturing centre to which they can be brought. Hitherto they may have come to England and other old countries of Europe in part because coal and iron were abundant there in juxtaposition; but the habit once set up, there seems no reason why they should not concentrate themselves on the old manufacturing centres. The ruder parts of the coal and iron industry may be attracted to other places, but the higher branches of manufacturing will be at no disadvantage if carried on at the old centres.

On the other hand, the old centres will retain the advantages, which are obviously very great, of climate, accumulated wealth, acquired skill, and concentration of population. That population under the new conditions is to go from them merely because they do not grow food which can be transported to them at the cost of a mere fraction of the aggregate income, and because they have not coal and iron in abundance and in juxtaposition, that abundance and juxtaposition, owing again to the diminished cost of conveyance, being no longer so indispensable as it was to the higher branches of manufacturing, appears certainly to be a 'large order.' What I have to suggest most strongly at any rate is that the advantages I have spoken of as possessed by old manufacturing centres are not unlikely to tell more and more under the new conditions, and that the indispensability of coal and iron is no longer to be spoken of as what it has been in the last century, during which apparently England owed so much of its precedence in manufacturing power to these causes.

To the same effect we may urge the specially great increase of the efficiency of coal in recent years. Cheap coal *in situ* cannot be relatively so important as it was in days when five or ten tons of coal were required to do the work which can now be done by one.

The truth is that the whole change that has been occurring is only a continuation of much larger historical changes. There has almost always in English history been some one industry that was supposed to be king. In the middle ages it was the growth and export of raw wool; last century it was the woollen manufacture itself; early in this century and down to a very late date cotton was



king; more lately, since the beginning of the railway and steamship era, it has been coal and iron. How do we know, how can we know, that coal and iron are to reign indefinitely, any more than wool, or the woollen manufacture, or cotton themselves have done? Changes are always going on, and for that reason I believe we should attach the more importance to the increasing signs that it is no longer necessary or indispensable for prosperous communities to live where their food and raw materials are grown—that there may be advantages of climate, of accumulated wealth, of acquired skill, of concentration of population which are now, under the new conditions, overwhelmingly more important. It would be absurd to dogmatise in such a matter. I hope, however, I have said enough to those who care to reflect to satisfy them that the indispensability even of coal and iron to the continuance of our material growth is no longer to be assumed, that there are wholly new conditions to be considered.

To come back to the practical point in all this discussion. Not only is there no sign in anything that has yet happened that the apparent check to our former rate of material growth is due to the loss of natural advantages which we once possessed, but the theory of natural advantages itself requires to be revised. Equally in this way as in the other ways that have been discussed, it is impossible to account for the apparent check to the former rate of our material growth which has been observed.

Having carried matters so far, however, and having found the insufficiency of the various causes which have been assigned for the check to our former rate of material growth, because they have not produced the sort of effect in detail which they ought to have produced so as to lead to the general effect alleged, or because they existed quite as much when the rate of growth was great as in recent years when a diminution has apparently been observed, it would seem expedient to inquire whether, in spite of the accumulation of signs to that effect, the apparent check to our rate of growth may, after all, not be a real one. To some extent I think we must conclude that this is the case. There are other facts which are inconsistent with a real and permanent check such as has been in question, and a general explanation of the special phenomena of arrest seems possible without supposing any such real check.

The first broad fact that does not seem quite reconcilable with the fact of a real diminution of the kind alleged in the rate of material growth generally is the real as distinguished from the apparent growth of the income tax assessments when allowance is made for the fall of prices which affect, as we have seen, all aggregate values. Assuming the fall of prices to be about 20 per cent., then we must add one-fourth to the assessments in 1885 to get the proper figure for comparison with 1875. The total of 631 millions for 1885 would thus become 787 millions, which is a falling-off of 35 millions, or 4 per cent. only, from the figure of 822 millions, which should have been reached if the rate of growth had been the same between 1875 and 1885 as between 1865 and 1875. Allowing for the raising of the lower limit of the income tax in the interval, this is really no decrease at all.

Of course this comparison may be thrown out if we are to assume the difference made by the fall of prices on the income tax assessments to be 15 or 10 per cent. only, instead of 20 per cent. But a point like this would involve a most elaborate discussion, for which this address would hardly be the occasion. I hope to find a better opportunity shortly in a continuation of my essay of ten years ago on the Accumulations of Capital in the United Kingdom. There is no doubt, however, that an allowance must be made for the difference of prices, and when any such allowance is made the rate of material growth would not appear to be so very much less between 1875 and 1885 than in the period just before, as it does in the above figures.

Another broad fact not easily reconcilable with the fact of a great diminution in the real rate of material growth in the last ten years is the steadiness of the increase of population and the absence of any sign, such as an increase in the proportion of pauperism, indicating that the people are less fully employed than they were. The increasing numbers must either be employed or unemployed, and if

there is an increase in the proportion of the unemployed the fact should be revealed in the returns of pauperism somehow. The existence of trade unions, no doubt, prevents many workmen coming on the rates who might formerly have done so, but there are large masses of workmen, the most likely to feel the brunt of want of employment, to whom this explanation would not apply.

What we find, however, is that population has increased as follows: between 1855 and 1865 from 27,800,000 to 29,900,000, or  $7\frac{1}{2}$  per cent.; between 1865 and 1875 from 29,900,000 to 32,800,000, or nearly 10 per cent.; and between 1875 and 1885 from 32,800,000 to 36,300,000, or over 10 per cent. If it is considered that the figures are not fairly comparable for the early period, owing to the specially large emigration from Ireland, which took away from the apparent numbers of the United Kingdom as a whole, but still allowed of as great an increase in the manufacturing parts of the country as there has been later, then we may take the figures for England only, and what we find is—between 1855 and 1865 an increase from 18,800,000 to 21,100,000, or  $12\frac{1}{2}$  per cent.; between 1865 and 1875 from 21,100,000 to 24,000,000, or nearly 14 per cent.; and between 1875 and 1885 from 24,000,000 to 27,500,000, or  $14\frac{1}{2}$  per cent. Whether, therefore, we take the figures for the United Kingdom or for England only, what we find is a greater increase of population in the last ten years than in either of the previous decades when the rate of material growth seemed so much greater. If there had been such real diminution in the rate of material growth, ought there not to have been some increase in the want of employment and in pauperism to correspond?

It is one of the most notorious facts of the case, however, that there has been no increase, but instead a very steady decrease of pauperism, excepting in Ireland, which is so small, however, as not to affect the general result. As regards England the figures are very striking indeed. The average number of paupers and proportion to population have been as follows in quinquennial periods in England since 1855:—

|                   | Number of<br>Paupers | Proportion to<br>Population per Cent. |
|-------------------|----------------------|---------------------------------------|
| 1855-59 . . . . . | 895,000              | 4.7                                   |
| 1860-64 . . . . . | 948,000              | 4.7                                   |
| 1865-69 . . . . . | 962,000              | 4.5                                   |
| 1870-74 . . . . . | 952,000              | 4.2                                   |
| 1875-79 . . . . . | 753,000              | 3.1                                   |
| 1880-84 . . . . . | 787,000              | 3.0                                   |

Thus there has been a steady diminution in the proportion to the population all through, accompanied by a diminution in the absolute numbers between 1865-69 and 1875-79, though there has since been a slight increase. In spite of all that can be urged as to a more stringent poor-law administration having made all the difference, it is difficult to believe that a real falling-off of a serious kind in the rate of our material growth in late years as compared with the period just before should not have led to some real increase of pauperism. Change of administration may do much, but it cannot alter the effect of any serious increase in the want of employment in a country.

The corresponding figures as to Scotland are much the same:—

|                   | Number of<br>Paupers | Proportion to<br>Population per Cent. |
|-------------------|----------------------|---------------------------------------|
| 1855-59 . . . . . | 123,000              | 4.2                                   |
| 1860-64 . . . . . | 125,000              | 4.2                                   |
| 1865-69 . . . . . | 131,000              | 4.3                                   |
| 1870-74 . . . . . | 123,000              | 3.7                                   |
| 1875-79 . . . . . | 103,000              | 2.9                                   |
| 1880-84 . . . . . | 100,000              | 2.7                                   |

Here there is the same steady diminution in the proportion of pauperism to population all through as we have seen in the case of England, accompanied in this case by a steady diminution of the absolute number of paupers since 1865-69.



The Scotch administration has been totally independent of the English, but the same results are produced.

In Ireland, as already hinted, the history has been different. There has been an increase in the pauperism accompanied by a decline of population. But Ireland is too small to affect the general result.

We are thus confronted by the fact that if there had been a real check of a serious kind to the rate of our material growth in the last ten years as compared with the ten years just before, there ought to have been some increase in the want of employment and in pauperism, but instead of there being such an increase there is a decline. The population apparently, while increasing even more rapidly in the last ten years than before, has been more fully employed than before. To make these facts consistent with a check to the rate of our material growth we must contrive some such hypothesis as that employment has been more diffused as regards numbers, but the aggregate amount of it has fallen off: another form of the hypothesis as to the effect of shorter hours of labour already discussed; but a little reflection will show that any such hypothesis is hardly admissible. It is difficult to imagine any change in the conditions of employment in so short a time which would make it possible for larger numbers to be employed along with a diminution in the aggregate amount of employment itself.

Another fact corresponding to this decrease of pauperism is the steady increase of savings bank deposits and depositors. These deposits are not, of course, the deposits of working classes only, technically so called. They include the smaller class of tradesmen and the lower middle classes generally. But, *quantum valeant*, the facts as to a growth of deposits and depositors should reflect the condition of the country generally in much the same way as the returns of pauperism. What we find then is, as regards deposits, that the increase between 1855 and 1865 was from 34,300,000*l.* to 45,300,000*l.*, or about one-third; between 1865 and 1875 from 45,300,000*l.* to 67,600,000*l.*, or about one-half; and between 1875 and 1885 from 67,600,000*l.* to 94,053,000*l.*, or just about 40 per cent.—a less increase than in the previous ten years, but not really less, perhaps, if allowance is made for the fall of prices in the interval, and in any case a very large increase. Then, as regards depositors, what we find is an increase between 1855 and 1865 from 1,304,000 to 2,079,000, or 59 per cent.; between 1865 and 1875 from 2,079,000 to 3,256,000, or 56 per cent.; and between 1875 and 1885 from 3,256,000 to over 5,000,000, or over 50 per cent. Whatever special explanations there may be, facts like these are at least not inconsistent with a fuller employment of the population in the last ten years than in the previous ten.

Yet another fact tending to the same conclusion may be referred to. The stationariness or slow growth of the income tax assessments in general in the last ten years, as compared with the rapid increase in the ten years just before, has already been referred to as one of the signs indicating a check in the rate of advance in our material growth. But when the returns are examined in detail there is one class of assessments, more significant, perhaps, than any, of the general condition of the nation, viz., houses, which is found to exhibit as great an increase in the last ten years as in the previous decade. Between 1865 and 1875 the increase in the item of houses in the income tax assessments in the United Kingdom was from 68,800,000*l.* to 94,600,000*l.*, or just about 37 per cent. In the following ten years the increase was from 94,600,000*l.* to 128,500,000*l.*, or just about 36 per cent. In 'houses,' then, as yet there is no sign of any check to the general rate of the material growth of the country. Allowing, in fact, for the great fall in prices in the last ten years, the real increase in houses would seem to have been more in the last ten years than in the ten years just before.

Other facts, such as the increase of Post Office business, may be referred to as tending to the same conclusion. But there is no need to multiply facts. If no hypothesis is to be accepted except one that reconciles all the facts, then these facts as to the increase of population, diminution of pauperism, increase of savings bank deposits and depositors, increase of houses must all be taken into account, as well as those signs as regards production and other factors, which have usually been most dwelt upon in discussing the question of the accumulation of wealth and the

material growth of the people. If the signs of a check to production in some directions can be reconciled with the fact of an unchecked continuance of the former rate of growth generally, then the later facts cited as to increase of population, diminution of pauperism, and the like, may be allowed to have their natural interpretation and to be conclusive on the point.

Such a general explanation, then, of the facts as to production in leading industries and the like, referred to in the earlier part of this address, consistent with the fact that there is no serious falling-off in the rate of our material growth generally, is to be found in the supposition that industry by a natural law is becoming more and more miscellaneous, and that as populations develop the disproportionate growth of the numbers employed in such miscellaneous industries, and in what may be called incorporeal functions, that is, as teachers, artists, and the like, prevents the increase of staple products continuing at the former rate. This supposition, it will be found, has a good deal to support it in the actual facts as to industry and population in recent years.

The foreign trade shows some sign of the change that is going on. Looking through the list of export articles some remarkable developments are to be noticed. The following short table speaks for itself:—

*Exports of the undermentioned Articles in the Years stated, with the Rates of Increase in 1855-65, 1865-75, and 1875-85 compared.*

|                                           | Quantities exported |            |       |       | Increase per Cent. |                   |         |
|-------------------------------------------|---------------------|------------|-------|-------|--------------------|-------------------|---------|
|                                           | 1855                | 1865       | 1875  | 1885  | 1855-65            | 1865-75           | 1875-85 |
| Candles, million lbs. . . . .             | 4                   | 4          | 5.3   | 7.8   | <i>Nil</i>         | 33                | 47      |
| Cordage and twine, thousand cwt.          | 110                 | 168        | 111   | 177   | 53                 | - 34 <sup>2</sup> | 59      |
| Plate glass, million sq. ft.              | 0.3                 | 0.6        | 1.6   | 3.9   | 100                | 166               | 143     |
| Jute yarn, million lbs. . . . .           | not stated          | 4.9        | 15.9  | 30.7  | —                  | 224               | 93      |
| Jute manufacture, million yds.            | „                   | 15.4       | 102.1 | 215   | —                  | 563               | 110     |
| Iron hoops, sheets, &c., thousand tons    | „                   | 116        | 204   | 331   | —                  | 76                | 62      |
| Tinned plates, thousand tons              | „                   | 63         | 138   | 298   | —                  | 119               | 116     |
| Other wrought iron, thousand tons         | „                   | 214        | 239   | 348   | —                  | 12                | 45      |
| Oil and floor cloth, million sq. yds.     | 0.5                 | 2.4        | 6.3   | 11.3  | 380                | 162               | 79      |
| Paper other than hangings, thousand cwt.  | 106 <sup>1</sup>    | 145        | 319   | 733   | 37                 | 120               | 130     |
| Dressed skins and furs, millions          | not stated          | not stated | 0.37  | 3.45  | —                  | —                 | 832     |
| Soap, thousand cwt.                       | 205                 | 140        | 251   | 402   | - 32 <sup>2</sup>  | 79                | 60      |
| Spirits, million gals.                    | 3.8                 | 2.0        | 1.0   | 2.7   | - 47 <sup>2</sup>  | - 50 <sup>2</sup> | 170     |
| Unenumerated, values } millions . . . . . | —                   | —          | £9.7  | £10.6 | —                  | —                 | 10      |

Thus there are not a few articles, of which jute is a conspicuous example, in which there has been an entirely new industry established within a comparatively short period; and, though the percentage of increase may not in all be so great in the last ten years as in the previous ten just because the industry is so wholly new, yet the amount of the increase is as great or greater. In other articles, such as soap and British spirits, there is a new start in the last ten years after a decline in the previous periods. Such cases as oil and floor cloth, paper other than hangings, and plate glass are also specially noticeable as practically new trades.

<sup>1</sup> 1858 not separately stated before.

<sup>2</sup> Decrease.



The list I am satisfied could be considerably extended, but I am giving it mainly by way of illustration. Finally, there is the item of other articles not separately specified—an item which is always changing in the statistical abstract because every few years one or more articles grow into sufficient importance to require separate mention, so that any extended comparison of this item for a long series of years is impossible. Still it is ever growing, and what we find in the last ten years is that, in spite of the fall of prices, the growth is from 9,700,000% to 10,600,000%, or nearly 10 per cent. Many of the articles referred to, it is plain, cannot run into much money, but the indications of a tendency are none the less clear. What is happening in the foreign trade is happening, we may be sure, in the home trade as well, of which in another way the increase in the imports of foreign manufactures, already referred to in another connection, is really a sign, as it implies the growth of miscellaneous wants among the consumers.

The census figures as to occupations tend, I believe, to confirm this observation as to the special growth of miscellaneous industries, but the discussion of the figures would require more preparation than I have had time for, and perhaps more space than can well be spared.

As to the growth of incorporeal functions, which is another fact significant of the supposed change in the direction of the employments of the people, I propose to appeal to the testimony of the census figures. I need refer on this head only to the paper read some time ago to the Statistical Society by Mr. Booth. Among those classes of population whose numbers in England and Wales in the last ten years have shown a disproportionate growth are the following:—

*Numbers and Percentage of Self-supporting Population employed.*

|                                  | Numbers |           | Percentage |      |
|----------------------------------|---------|-----------|------------|------|
|                                  | 1871    | 1881      | 1871       | 1881 |
| Transport . . . . .              | 524,000 | 654,000   | 4.9        | 5.6  |
| Commercial Class . . . . .       | 119,000 | 225,000   | 1.1        | 1.9  |
| Art and Amusement . . . . .      | 38,000  | 47,000    | 0.3        | 0.4  |
| Literature and Science . . . . . | 7,000   | 9,000     | —          | 0.1  |
| Education . . . . .              | 135,000 | 183,000   | 1.3        | 1.6  |
| Indefinite . . . . .             | 124,000 | 269,000   | 1.2        | 2.3  |
| Total . . . . .                  | 947,000 | 1,387,000 | 8.8        | 11.9 |

Following the indication of these figures, whatever qualification they may be subject to, we are apparently justified in saying that an increasing part of the population has been lately applied to the creation of incorporeal products. Their employment is industrial all the same. The products are consumed as they are produced, but the production is none the less real. If a nation chooses to produce more largely in this form as it becomes more prosperous, so that there is less development than was formerly the case in what were known as staple industries, it need not be becoming poorer for that reason; all that is happening is that its wealth and income are taking a different shape.

It is quite conceivable, then, and is in truth not improbable, that a check to the former rate of material growth in certain directions may have taken place of late years without any corresponding check to the rate of material growth generally, which would seem to be inconsistent with such facts as the growth of population, diminution of pauperism, increase of houses, and the like. The truth would seem to be that with the growth of staple industries, such as cotton, wool, coal, and iron, up to a point, there being reasons for the remarkably quick development of each for many years up to 1875, there comes a growth of new wants, the satisfaction of which drafts a portion of the national energy in new directions. Just because certain staples developed themselves greatly between 1855 and 1875 the time was likely to arrive when they would grow not quite so fast. For the

same reason the rapid increase for a certain period in the consumption per head of articles like sugar and tea was likely to be followed by a less rapid increase, the wants of consumers taking a new direction. Probably owing to the more and more miscellaneous character of modern industry, it will become more and more difficult to follow its development by dealing with staple articles only, while changes in aggregate values are untrustworthy as indications of real changes owing to changes in prices. Already there seems to be no doubt the staple articles are no longer a sufficient indication.

A supplementary explanation may be added which helps to explain another difficulty in the matter by which people are puzzled. I can imagine them saying that it is all very well to pooh-pooh the non-increase or slower increase of the production of staple articles and to assume that industry is becoming more and more miscellaneous; but other countries go on increasing their production of these same staple articles. The increase of the manufactures of cotton, wool, coal, and iron in Germany and the United States, they will say, has in recent years been greater in proportion than in England, which is undoubtedly true. The explanation I have to suggest, however, is that the competition with the leading manufacturing country, which England still is, is naturally in the staple articles where manufacturing has been reduced to a system, the newer and more difficult manufactures and the newer developments of industry generally falling as a rule to the older country. Even in foreign countries, however, there are signs of slower growth of recent years in the staple articles as compared with the period just before. In Germany, for instance, the production of coal increased between 1860 and 1866 [I take the years which I find available in Dr. Neumann Spallart's 'Uebersichten'] from 12,300,000 tons to 28,200,000, or nearly 129 per cent.; between 1866 and 1876 the increase was from the figure stated to about 50,000,000 tons, or about 77 per cent. only; between 1876 and 1885, another period of ten years, from the figure stated to 74,000,000 tons, or less than 50 per cent.—a rapidly diminishing rate of increase. In the United States of America the corresponding figures for coal are 15, 22, 50, and 103 million tons, showing a greater increase than in Germany, but still a rather less rate of increase since 1876 than in the ten years before. The experience as to the iron production would seem to be different, the increase in the United States and Germany having been enormously rapid in the last ten years; but I have not been able here to carry the figures far enough back for comparison. Still the facts as to coal in Germany are enough to show how rapidly the rate of increase of growth may fall off when a point is reached, and that the experience of the United Kingdom is by no means exceptional. As the staple articles develop abroad the rate of increase in such articles will diminish too, and foreign industry in turn will become more and more miscellaneous.

The conclusion would thus be that there is nothing unaccountable in the course of industry in the United Kingdom in the last ten years. In certain staple industries the rate of increase has been less than it was in the ten years just before, but there would seem to have been no increase or little increase in the want of employment generally, while there is reason to believe that certain miscellaneous industries have grown at a greater rate than the staple industries or have grown into wholly new being, and that there has also been some diversion of industry in directions where the products are incorporeal. These facts also correspond with what is going on abroad, a tendency to decline in the rate of increase of staple articles of production being general, and industry everywhere following the law of becoming more miscellaneous. Abroad also, we may be sure, as nations increase in wealth the diversion of industry in directions where the products are incorporeal will also take place. What the whole facts seem to bring out, therefore, is a change in the direction of industry of a most interesting kind. If we are to believe that the progress of invention and of the application of invention to human wants continues and increases, no other explanation seems possible of the apparent check to the rate of material growth which seems to be so nearly demonstrated by some of the statistics most commonly appealed to in such questions.

At the same time I must apply the remark which I applied at the earlier stage to the opposite conclusion that there had been a real check to the rate of increase



in our material growth. When the main statistics bearing on a particular point all indicate the same conclusion, it is not difficult to reason from them and to convince all who study them; but when the indications are apparently in conflict it would be folly to dogmatise. I have indicated frankly my own opinion, but I, for one, should like the subject to be more fully thrashed out. It is a very obvious suggestion, moreover, that one may prove too much by such figures—that it is an outrage on common sense to talk of there being no check to the rate of growth in the country when times are notoriously bad and everybody is talking of want of profit. What I should suggest finally, by way of a hypothesis reconciling all the facts, would be that probably there is some check to the rate of material growth in the last ten years, though not of the serious character implied by the first set of figures discussed; that this check may even be too small to be measured by general statistics though it is sufficient to account for no small amount of *malaise*; and that the *malaise* itself is largely accounted for, as I have suggested on a former occasion, by the mere fall of prices, whatever the cause, as it involves a great redistribution of wealth and income, and makes very many people feel poorer, including many who are not really poorer, but only seem so, and many who are really richer if they only allowed properly for the increased purchasing power of their wealth. All these facts are quite consistent with the fact of a very slight real diminution in the rate of our material growth generally, and with that change in the direction of the national industry, significant of a general change beginning throughout the world which would seem to have occurred.

To some extent also it ought to be allowed that the tendency in the very latest years seems unsatisfactory, and that the developments of the next few years should be carefully watched. Up to now there is nothing really alarming in the statistics when they are analysed and compared. It may be the case, though I do not think it is the case, that causes are in operation to produce that great check and retrogression which have not as yet occurred, though many have talked as if they had occurred. The exact limits of the discussion should be carefully kept in mind.

Fortunately, however, there is no doubt what some of the conclusions on practical points should be. If it be the case that the hold of an old country like England on certain staple industries of the world is less firm than it was, and, as I believe, must be less and less firm from period to period, owing to the natural development of foreign countries and the room there is among ourselves for development in new directions, then we should make assurance doubly sure that the country is really developing in new directions. If our dependence must be on the new advantages that have been described, such as acquired manufacturing skill, concentration of population, and the like, then we must make sure of the skill and of the best conditions of existence for the concentrated population. If, in point of fact, shorter hours of labour and taking things easy have contributed to check our rate of progress slightly, there is all the more reason for improving the human agent in industry so as to make work in the shorter hours more efficient. Looking at the stir there now is about technical education and such matters, and the hereditary character of our population, I see no cause to doubt that the future will be even more prosperous than the past. The national life seems as fresh and vigorous as ever. The unrest and complaints of the last few years are not bad signs. But the new conditions must be fully recognised. The utmost energy, mobility, and resource must be applied in every direction if we are only to hold our own.





MANCHESTER, 1887.

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ADDRESS  
TO THE  
MECHANICAL SCIENCE SECTION  
OF THE  
BRITISH ASSOCIATION.

BY

Professor OSBORNE REYNOLDS, M.A., LL.D., F.R.S., M.Inst.C.E.,

PRESIDENT OF THE SECTION.

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AT a meeting of the British Association in Manchester the subjects of interest to the members of this Section are sure to be numerous, and the attendance of those members whose opinions on the various subjects presented the Section will like to hear is sure to be such that every moment of the time at the disposal of the Section will be well occupied. It is also particularly undesirable to prolong the sittings, and so reduce the opportunities of visiting the exhibition and numerous works which abound with things which cannot fail to be of intense interest to members of this Section.

For these reasons I feel extremely unwilling to occupy the time of the Section with more than the briefest remarks by way of an address. Indeed, were it not that when in this chair in 1872 Sir Frederick Bramwell laid down the rule that for the President to break the custom of an address would be to show disrespect to the Section, I should have felt justified in consulting my inclination and proceeding at once with the regular work which lies before us.

It is now twenty-six years since the last meeting of this Section was held in Manchester, and it certainly seems fitting that in an address on this occasion something should be said as to the achievements in mechanical science accomplished in the interval. I wish sincerely that the task had fallen to some of you gentlemen whose far greater experience and power of expression would have enabled you to do justice to the subject. But under the circumstances I can only ask you to take it as a mark of my extreme respect for the Section, and proof of the appreciation in which I hold the honour conferred upon me in placing me in this chair, that I venture as a matter of duty to make a few remarks of the inadequacy of which I am only too conscious.

It is always difficult to arrive at a just appreciation of the relative importance of the events of our own time; and in any endeavour to review or take stock of the mechanical advance of the last quarter of a century, during which time things mechanical have divided the attention of the civilised world with matters political, it seems very necessary to remember that as the mechanical age gets older its relative activity is not to be gauged by the relative number and importance of such epoch-marking mechanical departures as compared with those which have distinguished past periods.

1887.

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If you recall—and again, to quote Sir Frederick Bramwell, the only purpose of an address is to force you to recall what you already know—in 1861 not only had we railways, ocean steam ships, including the ‘Great Eastern,’ still the giant of the tribe, a complete system of machinery for cotton and textile fabrics, besides the steam hammer, Armstrong’s accumulator, and types of all machine tools, but also one attempt had been made to lay an Atlantic cable; the Suez Canal was in course of construction; if not perfected, the Bessemer process was in use; as were steam ploughs, steam thrashing machines, reaping machines, and other agricultural machinery; we had also monster ironclads and rifled ordnance.

As new departures since 1861 which have already established themselves we have the telephone, the incandescent electric light, the dynamo, and the secondary battery, the gas engine and sewing machine, not to mention the bicycle. We have also the tin can and freezing machine and roller mills, as well as the machine gun and Whitehead torpedo.

One of these departures, the telephone, both from its usefulness and from the scientific interests which surround it, as affording, like the telescope, a means of directly increasing the power and range of one of our senses, must for ever remain recognised as a step in mechanical science, for the introduction of which this period will be distinguished.

The sewing machine, too, though little calculated to attract notice, in its influence on the welfare and appearance of all grades of society yields in importance to few, if any, previous mechanical steps. While the process of preserving food by means of the tin can and its more striking contemporary, the freezing machine, direct results of the discoveries of Pasteur, have already opened up the food-producing resources of the whole world for the supply of the few chosen spots, and in doing so created a most welcome demand for further advance in the application of steam.

Great things have been and still are hoped from the electric departures which have interested us so much during the last few years; also of the gas engine, which has most usefully occupied ground for which the steam engine is not well adapted; and as to the importance of machine guns and torpedoes many will think the less the better.

However high or low an estimate we may form of the probable future importance of some of these inventions, and however much disappointment we may feel at the non-success which has attended some of the boldest and apparently most promising departures, such as the Crompton process for substituting a blast of coal-dust for the ordinary furnace, or Sir Henry Bessemer’s endeavours to prevent distressing motion at sea, there is still no ground for discouragement.

For whether or not this period be henceforth remarkable for what, to borrow language from Section D, may be called the origination of new mechanical species, is a small matter compared with the fact that it has undoubtedly been remarkable for unprecedented achievements in the development of higher states of organisation in those mechanical species which were already in existence.

There has never been a time in which mechanical revolutions have followed one another with such rapidity. In all the main departments of practical mechanics progress has been so rapid that appliances have been superseded long before reaching the term of their natural existence. There are some steamboats like the steel mail-boats between Dover and Calais still on the same service as in 1861, but very few, and only such as were then much in advance of their time. The Atlantic fleet of Royal Mail steamers has twice undergone complete revolution. Not only have the paddle-boats which constituted the Cunard line in 1861, and which included the ‘Scotia,’ then new, entirely disappeared off the line, but the iron screw steamers which displaced them have given place to the steel boats with compound engines—‘Servia,’ ‘Aurania,’ ‘Etruria,’ and ‘Umbria.’

In railway appliances the iron road has given place to that of steel, iron tires and locomotives to steel, the block system has become general, as have continuous brakes; while the carriages in which members have spent four and a quarter hours on their way from London to this meeting, although mostly still of the English plan, are very different in size and ease from those in which five and a half hours were spent in 1861.



In the works and mills the change is not less complete. It is, indeed, the change here that has not only rendered possible, but forced on the revolution in our means of communication. The great step in the production of steel was already taken in 1861, and great results were then anticipated; there were, however, doubts and difficulties, and it was not for some years that sufficient mastery was obtained over the detail of the manufacture and use of the new material to bring about the general revolution which has therefore only reached its height during the last few years. if, indeed, it is yet reached—certainly it is yet far from complete.

To turn for one moment to the last year. Since the last meeting of this Section in Birmingham, the second Tay Bridge has been completed, over two miles long, having occupied only five years in construction.

The Severn Tunnel, one of the most difficult pieces of engineering ever attempted, has been completed and opened for passenger traffic.

The Forth Bridge, that structure the very thought of which causes those who have seen the place to hold their breaths, and of which the relative size may be best realised from the fact that, held out in arms an eighth part of a mile long, at a height of 200 feet above the sea, as a mother might hold out an infant, are structures no less than the single spans of the Britannia Bridge, 400 feet long. This gigantic structure, the progress of which Section G has watched since the meeting at Southampton, has now attained its full height of 360 feet, although otherwise not by any means fully formed.

Nor, as you well know, is it by the completion and progress only of great undertakings that this year is marked in the annals of engineering. It will be memorable, particularly in this district, as the year of the commencement of the Manchester Ship Canal. This undertaking, for which there is no precedent in this country, has excited so much interest that it cannot be otherwise than a matter of congratulation that a paper descriptive of this work is to be read before this meeting by the engineer, Mr. Leader Williams.

The completion of the Tay Bridge, the Severn Tunnel, the progress of the Forth Bridge, and the commencement of the Manchester Ship Canal in one year and in one country is sufficient assurance that, as yet, there is no lack of enterprise or sign of falling-off in heroic undertakings; nor are these by any means the only signs of great mechanical activity, notwithstanding the continual complaints of commercial depression.

In one direction, in particular, after many years of progress, so slow as to be something like stagnation, there has been a decided advance. The steam engine is such a familiar institution, and has been for so long looked upon as the prime mover of our entire mechanical system, that anything which affects its welfare excites a deeper interest than would a mere mechanical advance. It was therefore with anything but a feeling of pure exultation that we heard and felt the force of predictions a very few years ago that the days of the supremacy of the steam engine were numbered, that it would soon be a thing of the past, only to be found in the museum, a relic like Newcomen's engine and the stone implements by which our children would gauge the depth of mechanical barbarism of the age from which they had emerged. If sentiment be allowed in relation to anything mechanical, it must be with a sense of relief that it is now perceived how, so far from succumbing in the competition with what threatened to be formidable rivals, the only effect has been to bring about an important step in that internal development of the steam engine, which has been long looked for, but the accomplishment of which had for so long baffled the utmost efforts to bring it to a practical issue that it was almost despaired of—at least until it should be brought about by that circumstance which we all dread, the scarcity of coal.

The uppermost step of this advance yet reached is represented by the triple and quadruple expansion engines. These engines, of which the first seem to have been the triple engines of the 'Propontis' in 1874, designed by Mr. Kirk, the next those of the steam yacht 'Isa,' by Messrs. Douglas and Grant in 1878, and the third those of the 'Aberdeen,' again by Mr. Kirk, in 1881, rapidly sprang into favour for cargo steamers, in which they have already proved of such advantage as to more than threaten the necessity of another revolution in steamships almost before the



last is complete. Each week brings the announcement of some new accomplishment in the use of higher ratios of expansion and higher pressures of steam, so that while 60 or 70 lbs. was the maximum three years ago, we now hear of 130, 150, and 175 lbs.; and it is impossible to say to what they have not been carried at the present moment, and with commercial success.

There can be no doubt but that this latest step, as well as those of the surface condenser, high-pressure boilers, and compound engines which led up to it have been the immediate results of the premium on economy of coal offered by the opening up of the long steam routes, first through the Suez Canal and recently round the Cape. But these steps must none the less be considered as the results of the unprecedented attention and labour, theoretical and practical, which has been devoted to this object during the last fifty years. They have been a result of the theoretical work of Carnot and Regnault, crowned by the great discoveries of Joule and Meyer, and the subsequent work of Rankine, Thomson, Clausens, and Hern, besides others, which about the commencement of the period I am speaking of accomplished that complete exposition of the principles underlying the internal economy of all heat engines which have since furnished incitation and guidance to practical efforts. And not less have they been a result of the many practical attempts which have in the meantime been made to introduce similar and equally effective developments in the steam engine without waiting till they were called forth by circumstances; as notable amongst which I may instance the labours and successes of Mr. Perkins, who has experimentally developed the organisation of the steam engine beyond any point it has commercially reached. Each and all of these efforts has undoubtedly taken part in that readiness to take the forward step, as soon as circumstances were favourable, which is as necessary to development as are the favourable circumstances themselves. The fact that a great advance has been made in the use of higher-class steam engines, while it is the most gratifying circumstance one could have to record, affords the greatest encouragement to all those numerous workers for mechanical advance whose work is good, yet who do not see its immediate effect. It also emphasises the lesson that the most perfect machine is that which is most perfectly adapted to the circumstances under which it has to work; and amongst these circumstances is efficient attendance, which involves sufficient knowledge of its requirements and familiarity with its detail on the part of those who have it in charge; and while in a process of gradual development this education is insured, in the case of a sudden step it is generally wanting.

How far the present advance towards the limits to economy which are theoretically evident may extend in the immediate future it would be dangerous to predict. The present rate is immense, and not by any means confined to the marine engine, although I am not aware of any other class of engine in which triple expansion has yet been adopted as a system. The recent compound pumping engines have attained to very high organisation; and even in those classes of engines where economy of coal is more a matter of morality than of proved commercial importance, as mill engines and locomotives, great activity is evident in adapting and substituting compound engines, so as to allow of the use of greater pressures and higher degrees of expansion. The slow breathing compound locomotive of Mr. Webb has drawn many members of this Association on their way to this meeting. Nor is the portable engine behind, as has been shown by the recent trials of the Royal Agricultural Society at York. The result of these trials cannot but offer the greatest encouragement to engine makers of all kinds in their attempt at higher organisation. It is indeed difficult to say which has been the most gratifying—the high state of economy which these trials have shown to be realised, or the reinstitution of the trials themselves after a lapse of twenty years, during which interval their non-continuance has called forth but one expression—that of regret.

These almost sudden steps towards the realisation of efforts now extending over a century, to bring higher developments of the steam engine into practical use, have not passed without notice. The interest and excitement amongst those more directly acquainted and concerned with the steam engine and the use of steam are probably such as have not existed since the very early days of the railway. It is not therefore as something likely to be new to the members of this Section that I



have dwelt upon it. Remembering that there was another subject other than actual mechanical achievements on which I was, as it were, in duty bound to say something, it seemed hopeless for me to attempt to touch on all the many advances towards a higher degree of organisation in mechanics which constitute the mechanical feature of our era. I therefore have chosen this decided movement of the prime mover as the most significant and most gratifying, besides being of a kind the full importance of which is not so likely to be generally apprehended until pointed out as the importance of advances such as the electrical and metallurgical, involving some new departure or novel application.

That the character and rate of recent mechanical advance are both exactly such as would be expected to follow, as the result of a deeper and broader knowledge of scientific methods and the principles involved, seems to be the very best proof of advance in that other side of mechanical science in which this Section takes interest, or, more correctly, for which it exists—the increase and spread of mechanical knowledge.

It is as impossible as it is unnecessary for me to comment on the *furor* to which the movement first for popular scientific and now for technical instruction has reached—bringing into existence, by means of South Kensington, a complete system of sensibly free elementary scientific education over the country. Then the City and Guilds Technical Schools, with a general system of examination, and culminating in a Parliamentary Commission on Technical Education, with the prospect of seeing its labours result in an Act of Parliament providing for absolutely free technical instruction.

Elementary education, whatever may be its subjects, must of necessity depend for its permanent existence on some source of higher knowledge in those subjects. Without raising such questions as whether there exist at present means of training efficient teachers in all the branches for which technical education is promised, or whether such means will be forthcoming as a result of the demand for teachers, I would recall to your attention the recent progress made towards a higher training in that branch of science which most directly relates to mechanical progress, and which, according to no less an authority than the late Professor Rankine, received its first impulse from the institution of Section G.

So long ago as 1855 Rankine, in his characteristically concise address, dwelt upon the good work which this Section was doing in making it known that the application of the laws and principles of abstract mechanics to the purposes of practical mechanics constitutes a science of itself; a science the knowledge of which is essential before a knowledge of mathematics and abstract science can be of use to the practical engineer or mechanic; and for this science he then and there claimed the name Applied Mechanics. As a proof of the influence of Section G in making known the usefulness of this science he instanced the apparent increase in the desire to profit by the lectures of the late Professor Lewis Gordon, which had taken place since the Section was instituted.

Professor Gordon, who held the chair of mechanics in Glasgow University, was the first in this country to collect and embody in his lectures, and subsequently in a text-book, the important though scattered results of individual efforts to found the laws of practical mechanics on exact science. And at the time Rankine was speaking, this chair, to which Rankine himself was called the same year, was the only chair in this country from which such lectures were given.

Since that time the appreciation of that science has steadily increased; other colleges took up the subject mostly as forming part of courses entitled engineering or naval science. Amongst these was Owens College, in which, not till after the last meeting in Manchester of this Association, the leading engineers founded and endowed, which is more important, the chair which it has been my fortune to occupy for nineteen years.

During the earlier part of this time both teachers and students were labouring under the disadvantage arising from the novelty of the subject—the former having to make an almost arbitrary selection of what they would teach, and the latter not knowing exactly what it was they were going to learn. Gradually, however, by the help of experience from the somewhat earlier French schools and with the



admirable works of Rankine as a foundation, the lectures or theoretical courses have become clear and distinct, while the advantage to be gained has become so generally recognised that of late years there has been almost a scramble to found new colleges to teach engineering or to introduce such teaching into existing colleges; and most satisfactory to those engaged in the introduction of this subject is the fact that it is from the engineers themselves that the interest and funds necessary for this work have come. Since 1867 the Owens College has received gifts and bequests from engineers, including those of highest standing in the neighbourhood, of upwards of 150,000*l*. In the same way at Sheffield and at Leeds, where, as is well known, an engineering school has just been founded by Sir John Hawkshaw and the engineers of the town, and again at Liverpool.

It cannot for one moment be doubted that this movement has been brought about by the conviction of the necessity of an education which, in its subjects and methods of teaching, is much more closely related than was the older system of the Universities, to the actual work which the students may eventually be called upon to undertake. That it is in fact evidence of the appreciation, by those having the greatest experience, of the necessity of higher scientific training for engineers. This is what engineering schools during their struggle for existence have endeavoured to supply. And in spite of the danger which seems to beset all schools as they become older, to fall into the academic or pure—not because it is the most desirable to be learnt, but because it is by far the easiest to teach—in spite of this danger, such in this case is the pressure from without, that it may be hoped the schools of engineering and applied science may be kept up to the mark, both in extending our knowledge of the laws and principles which more immediately underlie the results of practical experience in art, and in teaching the methods of most useful application; and that while encouraged to offer every inducement to the attainment of a sound knowledge of the principles, they will not be allowed to fall into the fatally easy errors of carrying the abstractions of this science outside all possible application, or blocking the way by the insistence on impossible preliminary attainments in mathematics and pure science.

To be hailed as one of the greatest inducements to keeping alive in engineering schools a real scientific interest in the practical work which is going on around them is the introduction of what are now called engineering laboratories, in which students may familiarise themselves with the actual subjects for which the theoretical work is undertaken, and have placed before them in their most naked forms the data and mechanical actions on which practical achievements depend, as well as being taught the use of all those instruments and methods of measurement which it is one of the first objects of these laboratories to extend and to perfect, and which measurements are now, as the result of a better knowledge of principles, rapidly displacing the older methods of arriving at conclusions in engineering.

It is to our Continental neighbours that we principally owe the origination of these laboratories as a means of research, but, as a system of instruction distinct from a workshop it owes much to Professor Kennedy, who was, I believe, the first to introduce the testing machine and regular engine trials as part of the regular course of instruction for students in engineering, under the title of a laboratory course. The want of such a course must, however, it would seem, have been severely felt, to judge by the rapidity with which Professor Kennedy's example has been followed in almost all the engineering schools in the country.

It is true that as adjuncts to academic institutions these laboratories can hardly be said to have passed the experimental stage, and it evidently remains to be seen whether when the present arrears of outstanding questions in engineering science are worked up, and the courses of instruction become stereotyped, sufficient variety of work will be found to justify the expense which, both as regards qualified instructors and maintenance of apparatus, must, as compared with the number of students receiving instruction, be greater than is general with academic instruction. At present, however, thanks to the liberality of engineers and their friends, there seems no ground for fear, each new laboratory being furnished with more complete and expensive apparatus than the last. During the erection and fitting of the Whitworth Laboratory in Owens College, which is only now on the verge of com-



pletion, it has been very impressing to see the goodwill shown toward the work by everybody who has had to do with it; the ready help of engineers of the greatest experience, like Mr. Rambottom and Mr. Robinson, who have spared neither time nor trouble in giving it the benefit of their experience; also by those who have undertaken the construction of the appliances, particularly Mr. William Mather, of Salford Iron Works, where neither trouble nor money has been considered in the efforts made to render the engines for the laboratory as perfectly adapted as possible to the very novel and numerous requirements. Taking this particular instance as evidence not only of the general feeling in favour of this movement, but also of the solid support it is to receive, one cannot help concluding that there is a great future before it; and that at last a method has been found of extending and spreading the higher knowledge of mechanical science which commends itself alike to the practical and theoretical.

Everyone who has paid attention to the history of mechanical progress must have been impressed by the smallness in number of recorded attempts to decide the broader questions in engineering by systematic experiments, as well as by the great results which in the long run have apparently followed as the effect of these few researches. I say apparently, because it is certain that there have been other researches which probably, on account of failure to attain some immediate object, have not been recorded, although they may have yielded valuable experience which, though not put on record, has, before it was forgotten, led to other attempts. But even discounting such lost researches, it is very evident that mechanical science was in the past very much hampered by the want of sufficient inducement to the undertaking of experiments to settle questions of the utmost importance to general advance, but which have not promised pecuniary returns—scientific questions which involved a greater sacrifice of time and money than individuals could afford. In recent periods the aid and encouragement which it has been one of the first objects of the British Association to afford such researches has led to many results of the greatest importance, both directly and indirectly, by the effect of example in calling forth aid from other institutions—that of mechanical engineers, for instance, which recently induced Mr. Tower to carry out his already celebrated research on ‘The Friction of Lubricated Journals,’ the results of which research certainly claim notice as constituting one of the most important of recent steps in mechanical science. Such investigations it is now the function as well as the interest of mechanical laboratories to undertake, and thus what has hitherto been a great obstacle in the path of mechanical progress seems in a fair way to be removed and steady advance to be insured.

To what all this may lead us it is no part of my undertaking to consider, but I venture to end this imperfect address on the progress of mechanical science during the past twenty-six years by what appears to me the most satisfactory conclusion—viz. that to such mechanical progress there is apparently no end: for, as in the past so in the future, each step in any direction will remove limits and carry us past barriers which have till then blocked the way in other directions; and so what for the time may appear to be a visible end or practical limit will turn out but a bend in the road.





MANCHESTER, 1887.

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ADDRESS  
TO THE  
ANTHROPOLOGICAL SECTION  
OF THE  
BRITISH ASSOCIATION,  
BY  
PROFESSOR A. H. SAYCE, M.A.,  
PRESIDENT OF THE SECTION.

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SURPRISE has sometimes been expressed that anthropology, the science of man, should have been the last of the sciences to come into being. But the fact is not so strange as it seems at first sight to be. Science originated in curiosity, and the curiosity of primitive man, like the curiosity of a child, was first exercised upon the objects around him. The fact that we are separate from the world about us, and that the world about us is our own creation, is a conviction which grows but slowly in the mind either of the individual or of the race in general. The child says, 'Charley likes this,' before he learns to say, '*I* like this,' and in most languages the objective case of the personal pronoun exhibits earlier forms than the nominative.

Moreover, it is only through the relations that exist between mankind and external nature that we can arrive at anything like a scientific knowledge of man. Science, it must be remembered, implies the discovery of general laws, and general laws are only possible if we deal, not with the single individual, but with individuals when grouped together in races, tribes, or communities. We can never take a photograph of the mind of an individual, but we can come to know the principles that govern the actions of bodies of men, and can employ the inductive method of science to discover the physical and moral characteristics of tribes and races. It is through the form of the skull, the nature of the language, the manners and customs, or the religious ideas of a people that we can gain a true conception of their history and character. The thinker who wishes to carry out the precept of the Delphian oracle and to 'know himself' must study himself as reflected in the community to which he belongs. The sum of the sciences which deal with the relations of the community to the external world will constitute the science of anthropology.

The field occupied by the science is a vast one, and the several workers in it must be content to cultivate portions of it only. The age of 'admirable Crichtons' is past; it would be impossible for a single student to cover with equal success the whole domain of anthropology. All that he can hope to do is to share the labour with others, and to concentrate his energies on but one or two departments in the wide field of research. A day may come when the work we have to perform will be accomplished, and our successors will reap the harvest that we have sown. But meanwhile we must each keep to our own special line of investigation, asking only that others whose studies have lain in a different direction shall help us with the results they have obtained.

I shall therefore make no apology for confining myself on the present occasion to those branches of anthropological study about which I know most. It is more particularly to the study of language, and the evidence we may derive from it as to the history and development of mankind, that I wish to direct your attention. It is in language that the thoughts and feelings of man are mirrored and embodied; it is through language that we learn the little we know about what is passing in the minds of others. Language is not only a means of intercommunication, it is also a record of the ideas and beliefs, the emotions and the hopes of the past generations of the world. In spoken language, accordingly, we may discover the fossilised records of early humanity, as well as the reflection of the thoughts that move the society of to-day. What fossils are to the geologist words are to the comparative philologist.

But we must be careful not to press the testimony of language beyond its legitimate limits. Language is essentially a social product, the creation of a community of men living together and moved by the same wants and desires. It is one of the chief bonds that bind a community together, and its existence and development depend upon the community to which it belongs. If the community is changed by conquest or intermarriage or any other cause the language of the community changes too. The individual who quits one community for another has at the same time to shift his language. The Frenchman who naturalises himself in England must acquire English; the negro who is born in the United States must adopt the language that is spoken there.

Language is thus a characteristic of a community, and not of an individual. The neglect of this fact has introduced untold mischief not only into philology, but into ethnology as well. Race and language have been confused together, and the fact that a man speaks a particular language has too often been assumed, in spite of daily experience, to prove that he belongs to a particular race. When scholars had discovered that the Sanskrit of India belonged to the same linguistic family as the European languages, they jumped to the conclusion that the dark-skinned Hindu and the light-haired Scandinavian must also belong to one and the same race. Time after time have I taken up books which sought to determine the racial affinities of savage or barbarous tribes by means of their language. Language and race, in short, have been used as synonymous terms.

The fallacy is still so common, still so frequently peeps out where we should least expect it, that I think it is hardly superfluous, even now, to draw attention to it. And yet we have only to look around us to see how contrary it is to all the facts of experience. We Englishmen are bound together by a common language, but the historian and the craniologist will alike tell us that the blood that runs in our veins is derived from a very various ancestry. Kelt and Teuton, Scandinavian and Roman have struggled together for the mastery in our island since it first came within the horizon of history, and in the remoter days of which history and tradition are silent archaeology assures us that there were yet other races who fought and mingled together. The Jews have wandered through the world adopting the languages of the peoples among whom they have settled, and in Transylvania they even look upon an old form of Spanish as their sacred tongue. The Cornishman now speaks English; is he on that account less of a Kelt than the Welshman or the Breton?

Language, however, is not wholly without value to the ethnologist. Though a common language it is not a test of race, it is a test of social contact. And social contact may mean—indeed very generally does mean—a certain amount of intermarriage as well. The penal laws passed against the Welsh in the fifteenth century were not sufficient to prevent marriages now and then between Welshmen and Englishmen, and in spite of the social ostracism of the negro in the Northern States of America intermarriages have taken place there between the black and the white population. But in the case of such intermarrying the racial traits of one member only of the union are, as a general rule, preserved. The physical and moral type of the stronger parent prevails in the end, though it is often not easy to tell beforehand on which side the strength will lie. Sometimes, indeed, the physical and moral characters are not inherited together, the child following one of his parents



in physical type while he inherits his moral and intellectual qualities from another. But even in such cases the types preserve a wonderful fixity, and testify to the difficulty of changing what we call the characteristics of race.

Herein lies one of the most obvious differences between race and language, a difference which is of itself sufficient to show how impossible it must be to argue from the one to the other. While the characteristics of race seem almost indelible, language is as fluctuating and variable as the waves of the sea. It is perpetually changing in the mouths of its speakers; nay, the individual can even forget the language of his childhood and acquire another which has not the remotest connection with it. A man cannot rid himself of the characteristics of race, but his language is like his clothing which he can strip off and change almost at will.

It seems to me that this is a fact of which only one explanation is possible. The distinctions of race must be older than the distinctions of language. On the monuments of Egypt, more than four thousand years ago, the Libyans are represented with the same fair European complexion as that of the modern Kabyles, and the painted tomb of Rekh-mâ-ra, a Theban prince who lived in the sixteenth century before our era, portrays the black-skinned negro, the olive-coloured Syrian, and the red-skinned Egyptian with all the physical peculiarities that distinguish their descendants to-day. The Egyptian language has ceased to be spoken even in its latest Coptic form, but the wooden figure of the 'Sheikh-el-beled' in the Bulaq Museum, carved 6,000 years ago, reproduces the features of many a *fellah* in the modern villages of the Nile. Within the limits of history racial characteristics have undergone no change.

I see, therefore, no escape from the conclusion that the chief distinctions of race were established long before man acquired language. If the statement made by M. de Mortillet is true, that the absence of the mental tubercle, or bony excrescence in which the tongue is inserted, in a skull of the Neanderthal type found at La Naulette, indicates an absence of the faculty of speech, one race at least of palæolithic man would have existed in Europe before it had as yet invented an articulate language. Indeed, it is difficult to believe that man has known how to speak for any very great length of time. On the one hand, it is true, languages may remain fixed and almost stationary for a long series of generations. Of this the Semitic languages afford a conspicuous example. Not only the very words, but even the very forms of grammar are still used by the Bedouin of Central Arabia that were employed by the Semitic Babylonians on their monuments five thousand years ago. At that early date the Semitic family of speech already existed with all its peculiarities, which have survived with but little alteration up to the present day. And when it is remembered that Old Egyptian, which comes before us as a literary and decaying language a thousand years earlier, was probably a sister of the parent Semitic speech, the period to which we must assign the formation and development of the latter cannot fall much short of ten thousand years before the Christian era. But on the other hand there is no language which does not bear upon its face the marks of its origin. We can still trace through the thin disguise of subsequent modifications and growth the elements, both lexical and grammatical, out of which language must have arisen. The Bushman dialects still preserve the inarticulate clicks which preceded articulate sounds in expressing ideas; behind the roots which the philologist discovers in allied groups of words lie, plainly visible, the imitations of natural sounds, or the instinctive utterances of human emotion; while the grammar of languages like Eskimaux or the Aztec of Mexico carries us back to the first mechanism for conveying the meaning of one speaker to another. The beginnings of articulate language are still too transparent to allow us to refer them to a very remote era. I once calculated that from thirty to forty thousand years is the utmost limit that we can allow to man as a speaking animal. In fact, the evidence that he is a drawing animal, derived from the pictured bones and horns of the palæolithic age, mounts back to a much earlier epoch than the evidence that he is a speaking animal.

Mr. Horatio Hale has lately started a very ingenious theory to account, not indeed for the origin of language in general, but for the origin of that vast number of apparently unallied families of speech which have existed in the world. He has come across examples of children who have invented and used languages of their own,



refusing at the same time to speak the language they heard around them. As the children belonged to civilised communities the languages they invented did not spread beyond themselves, and after a time were forgotten by their own inventors. In an uncivilised community, however, it is quite conceivable that such a language might continue to be used by the children after they had begun to grow up and be communicated by them to their descendants. In this case a wholly new language would be started, which would have no affinities with any other, and after splitting into dialects would become the parent of numerous derived tongues. I must confess that the evidence brought forward by Mr. Hale in support of his theory is not quite convincing to me. It has yet to be proved that the words used by the children to whom he refers were not echoes of the words used by their elders. If they were, a language that originated in them would show more signs of lexical affinity to the older language than is the case with one family of speech when compared with another. On the other hand, the theory would tend to throw light on the curious fact that the morphological divisions of language are also geographical.

By the morphology of a language I mean its structure, that is to say, the mode in which the relations of grammar are expressed in a sentence, and the order in which they occur. These vary considerably, the chief variations being represented by the polysynthetic languages of America, the isolating languages of Eastern Asia, the postfixal languages of Central Asia, the prefixal languages of Africa, and the inflectional languages of Europe and Western Asia. Now it will be observed that each of these classes of language is associated with a particular part of the globe, the isolating languages, for example, being practically confined to Eastern Asia, and the polysynthetic languages to America. Within each class there are numerous families of speech between which no relationship can be discovered beyond that of a common structure; they agree morphologically, but their grammar and lexicon show no signs of connection. If we adopt Mr. Hale's theory we might suppose that the genealogically distinct families of speech grew up in the way he describes, while their morphological agreement would be accounted for by the inherited tendency of the children to run their thinking into a particular mould. The words and contrivances of grammar would be new, the mental framework in which they were set would be an inheritance from former generations.

I have spoken of the inflectional languages as belonging to Europe and Western Asia. This is true if we give a somewhat wide extension to the term inflectional, and make it include not only the Indo-European group, but the Georgian and Semitic groups as well. But, strictly speaking, the Indo-European, or Aryan, languages have a structure of their own, which differs very markedly from that of either the Georgian or the Semitic families. The Semitic mode of expressing the relations of grammar by changing the vowels within a framework of consonants differs as much from the Aryan mode of expressing them by means of suffixes as does the Semitic partiality for words of three consonants from the Indo-European carelessness about the number of syllables in a word. Though it is quite true that the Semitic languages at times approach the Indo-European by using suffixes to denote the forms of grammar, while at other times the Indo-European languages may substitute internal vowel change for external flexion, nevertheless, in general, the kind of flexion employed by the two families of speech is of a totally different character.

This difference of structure, coupled with a complete difference in phonology, grammar, and lexicon, has always seemed to me to negative the attempts that have been made to connect the Aryan and Semitic families of language together. The attempts have usually been based on the old confusion between language and race; both Aryans and Semites belong to the white race; therefore it was assumed their languages must be akin. As long as it was generally agreed that the primitive home of the Aryan languages was, like that of the Semitic languages, the western part of Asia, the confusion was excusable. If the earliest seats of the speakers of each were in geographical proximity, there was some reason for believing that languages which were alike spoken by members of the white race, and were alike classed as inflectional, would, when properly questioned, show signs of a common origin.



But that general agreement no longer exists. While the Asiatic origin of the Semitic languages is beyond dispute, scholars have of late years been coming more and more to the conclusion that Europe was the cradle of the Aryan tongues. Their European origin was first advocated by our countryman Dr. Latham, and was subsequently defended by the eminent comparative philologist Dr. Benfey; but it is only within the last half-dozen years that the theory has won its way to scientific recognition. Different lines of research have been converging towards the same result, and indicating north-eastern Europe as the starting-point of the Indo-European languages, while the evidences invoked in favour of their Asiatic origin have one and all broken down.

These evidences chiefly rested on the supposed superiority of Sanskrit over the other Indo-European languages as a representative of the parent-speech from which they were all descended. The grammar and phonology of Sanskrit were imagined to be more archaic, more faithful to the primitive pattern than those of its sister-tongues. It was argued that this implied a less amount of migration and change on the part of its speakers, a nearer residence, in fact, to the region where the parent-speech had once been spoken. As a comparison of the words denoting certain objects in the Indo-European languages showed that this region must have had a cold climate, it was placed on the slopes of the Hindu-Kush or at the sources of the Oxus and Jaxartes.

But we now know that instead of being the most faithful representative of the parent-speech, Sanskrit is in many respects far less so than are its sister-languages of Europe. Its vocabulary, for instance, has been thrown into confusion by the coalescence of the three primitive vowel sounds *ā*, *ē*, *ō* into the single monotonous *ā*, a corruption which is paralleled by the coalescence of so many vowels in modern cultivated English in the so-called 'neutral' *ə*. Greek, or even the Lithuanian, which may still be heard to-day from the lips of unlettered peasants, has preserved more faithfully than the Sanskrit of India the features of the parent Aryan. If the faithfulness of the record is any proof of the geographical proximity of one of the Indo-European languages to their common mother, it is in the neighbourhood of Lithuania, rather than in the neighbourhood of India, that we ought to look for traces of the first home of the Aryan family.

But the theory of the Asiatic origin of the Indo-European family has not only been deprived of its main support by the dethronement of Sanskrit, and the transfer of its primacy to the languages of Europe, what Professor Max Müller has termed 'linguistic palæontology' has further assisted in overthrowing the crumbling edifice. When we find words of similar phonetic form and similar meaning in both the Asiatic and the European branches of the Aryan family—words, too, which it can be shown have not been borrowed by one Indo-European language from another—we are justified in concluding that the objects or phenomena denoted by them were already known to the speakers of the parent language. When we find, for instance, that the birch is known by the same name in both Sanskrit and Teutonic, we may infer that it was a tree with which the speakers of the mother tongue of Sanskrit and Teutonic were acquainted, and that consequently they must have lived in a cold climate. In Europe that would have been westward of a line drawn from Königsberg to the Crimea, to the east of which the birch tree does not grow.

Four years ago a valuable contribution to the linguistic palæontology of the Aryan languages was made by Professor Otto Schrader. For the first time the question was approached from the present level of comparative philology, and all words were excluded from comparison which did not satisfy the requirements of phonetic law. The results were sadly disquieting to the believers in that idyllic picture of primitive Aryan life to which we had so long been accustomed. Professor Schrader proved that the speakers of the parent Aryan language must not only have lived in a cold climate—a fact which was known already—but that they must have lived in the stone age, with the skins of wild beasts only to protect them from the rigours of the winter, and nothing better than stone weapons with which to ward off the attacks of savage animals. Their general culture was on a level with their general surroundings. It was little better than that of the Fuegian before he came into contact with European missionaries. The minuteness with which the varying



degrees of family relationship were named, instead of indicating an advanced social life, as was formerly imagined, really indicated the direct contrary. The primitive Aryan was indeed acquainted with fire; he could even sew his skins together by means of needles of bone; and possibly could spin a little with the help of rude spindle-whorls; but beyond this his knowledge of the arts does not seem to have extended. If he made use of gold or meteoric iron, it was only of the unwrought pieces which he picked up from the ground and employed as ornaments; of the working of metals he was entirely ignorant. But he already practised a kind of rude agriculture, though the art of grinding corn was as yet unknown, and crushed spelt was eaten instead of bread; while the community to which he belonged was essentially that of pastoral nomads, who changed from season to season the miserable beehive huts of wattled mud in which they lived. They could count at least as far as a hundred, and believed in a multitude of ghosts and goblins, making offerings to the dead, and seeing in the bright sky a potent deity.

In calling the speaker of the Aryan parent speech the primitive Aryan I must not be supposed to be prejudging the question as to the particular race to which he belonged. This is a question which has recently been handled with great ability by an Austrian anthropologist—Dr. Karl Penka. In a remarkable book, published at the end of last year, he endeavours to substantiate the hypothesis advanced in an earlier work, and to show that the first speakers of the Aryan languages were the fair-haired, blue-eyed, light-complexioned dolichocephalic race, which is still found in its greatest purity in Scandinavia; that it was this race which in the neolithic period spread southwards, imposing its yoke upon subject populations, like the Norsemen and Normans of later days, and carrying with it the dialects, which afterwards developed into the Aryan languages; and that, finally, it was the same race which in the remote days of the palæolithic age inhabited western and central Europe, where it has left its remains in the typical skulls of Cannstatt and Engis. Dr. Penka would ascribe to its long residence in the semi-arctic climate of palæolithic Europe the permanent blanching of its skin and hair—a form of albinism which Dr. Poesche in 1878 endeavoured to explain by the climatic conditions of the Rokitno marshes in Russia, where he placed the cradle of the white Aryan race.

It cannot be denied that all the probabilities are at present on Dr. Penka's side, so far as his main contention is concerned. Without denying that the speakers of the Aryan parent speech may have already included slaves or wives of alien race, it is probable that the majority of them were of one blood. They formed a single community, nomad it is true, and therefore less likely to mix with foreigners, but still sufficiently a single community to speak a language the several dialects of which were so alike as to be mutually intelligible. In the social condition in which the speakers were, and in an age when the waste lands of the world were still extensive, the greater part of such a community must necessarily, we should think, have belonged to the same race. The evidences of language, moreover, as we have seen, point to a cold and northerly climate as the original seat of the community; and since they further inform us that the birch was known to it, we may conclude that this climate lay westward of Königsberg and Russia. Penka has striven to show that the animals whose bones or shells are found in the Scandinavian kitchen-middens are just those whose names are common to the Indo-European languages, or at all events the European section of the latter. Now, the skulls disinterred from the prehistoric burial-places of Denmark and the southern districts of Sweden and Norway are, for the most part, identical with the skulls still characteristic of the Scandinavian population where they accompany a fair skin and light hair and eyes. By combining these two facts we arrive at the conclusion that the fair Scandinavian race is the modern descendant of the race which spoke the parent language of the primitive Aryan community, and left traces of itself in the Scandinavian kitchen-middens. The conclusion is supported by the testimony of history. On the one hand, we have the testimony of classical writers that the Aryan-speaking Kelts of the Christian era were not the dark small-limbed population which now occupies the larger part of France, but men of large stature, with the blue eyes and fair hair of their Teutonic brethren; while the ideal specimens of humanity conceived



of by the aristocratic art of Italy and Greece were the golden-haired Apollo and the blue-eyed Athênê. On the other hand, it was from Scandinavia that in later times other bands of warriors poured forth, who made their way into the countries of the Mediterranean, and even Asia, and established themselves as conquering aristocracies in the midst of subject populations. The Kelts succeeded in reaching Asia Minor, the Scando-German hordes overthrew the Roman empire, the Northmen established themselves from Russia on the east to Iceland and Greenland on the west, and the Normans made Sicily their own long before the days of the German Frederick. The only point in which the later historical irruptions of the Scandinavian peoples differed from their prehistoric ones was, that while the later irruptions were made by sea, the older were made by land. The sail was unknown to the tribes of the north until the age of their intercourse with the Romans, from whom they borrowed both the conception and the name of the *sagulum*, or 'sail.' The course of their migrations must have followed the valleys or the great rivers.

If southern Scandinavia is thus to be regarded as the original home of the Aryan languages, and the race which first spoke those languages, and which we may therefore call Aryan, is to be identified with the Scandinavian type, it follows that the further south and east we advance from this primary starting-point the less pure will the type become. It will be in the neighbourhood of that starting-point and in northern Europe that we shall expect to find the largest number of undiluted Aryan languages and the purest examples of the Aryan breed. In Greece and Armenia, in Persia and India we must look for mixture and decay. And such indeed is the fact. Mr. Wharton has found, by a careful analysis of the Greek lexicon, that out of 2,740 primary words only 1,580 can be referred with any probability to an Indo-European origin, while the prevailing racial type in ancient as in modern Greece was distinctly non-Aryan. Indeed, I am inclined to believe that the culture revealed by the excavations at Mykênæ, Tiryns, and on other prehistoric Greek sites belonged not to a Hellenic but to a pre-Hellenic population, and that the Aryan Greeks first made their appearance in Hellas at the epoch of what later tradition called the Dorian immigration. It was to the north that Greek legends pointed as the primæval home of the Hellenic race and civilisation, and Dôdôna ever continued to be revered as the oldest sanctuary of the Hellenic world. In India it is notorious that the Aryan-speaking Hindus entered the country from the north-west, and failed to spread far into the burning plains of the south. The date of their invasion is uncertain, but for myself I have grave doubts whether it was earlier than the eighth or even the seventh century B.C. At all events it was not until after the seventh century B.C., as we now know from the express testimony of the cuneiform inscriptions of Van, that the Aryan-speaking Armenians entered the land which now bears their name, and recent philological researches have confirmed the assertion of Greek writers that the Armenians were a colony of the Phrygians who had themselves emigrated from Thrace. Up to the closing days of the Assyrian empire the monuments make it clear that no Aryans had as yet settled between the Kurdish ranges on the east and the Halys on the west.

But while the extension into Asia of what I will now, following Penka's example, call the Aryan race, seems to be referred to a comparatively recent period, there is a curious fact which goes to show that the same, or a closely allied, race once spread along the northern coast of Africa. On Egyptian monuments, which date back to the sixteenth century before our era, the Libyan tribes of this district are described and depicted as white. Their descendants are still to be found in the mountainous parts of the coast, those of Algeria being commonly known under the name of Kabyles. I saw a good deal of them last winter, and must confess to being greatly struck by their appearance. I had known, of course, that they belonged to the white race and were characterised by blue eyes and light hair, but I was not prepared to find that their complexion was of that transparent whiteness which freckles readily and is supposed to mark the so-called red Kelt. They are dolichocephalic, and as their skulls agree with those discovered in the prehistoric cromlechs of Roknia and other places it is plain that their distinctive features are not due, as was formerly supposed, to intermixture with the Vandals.

The cromlechs in which they once buried their dead are quite as remarkable as their physical characteristics. Cromlechs of a similar shape are found extending through Spain and western France to the northern portion of the British Isles. Since dolichocephalic skulls occur in connection with them, while the physical characteristics of the modern Kabyle resemble so strikingly those of a particular portion of the modern Irish population, we seem driven to infer that the Kabyle and the 'red Kelt' are alike fragments of a race that once spread from Scotland and Ireland to the northern coast of Africa and interred its dead in chambers formed of five large blocks of stone. Though the custom of burying in these cromlechs continued into the bronze age, the majority of them go back to the neolithic period.

Are we to suppose, then, that one stream of Aryan immigrants, after making its way to the west, wandered along the western coast of Europe, and eventually crossed the Straits of Gibraltar and took possession of Africa? Or are we to believe that the Aryan race of southern Scandinavia was allied in blood, though not in language, with a population which inhabited the extreme west of Europe, and had, it may be, at the close of the glacial epoch, passed over to the neighbouring mountains of Africa? It must be remembered that the Kabyle complexion is not precisely the same as that of the Scandinavian. Both are white, but the skin of the one has a semitransparent appearance, while the whiteness of the other may be described as mealy. It will be worth while to determine whether between the dolichocephalism of the Kabyle and the dolichocephalism of the Scandinavian any distinction can be drawn.

The question has a bearing on the origin of a part of our own population. I have already compared the Kabyle with the 'red Kelt.' But the expression 'red Kelt,' like most popular expressions, is by no means exact. It confuses in one two distinct types. The large-limbed, red-haired Highlander, who calls to mind the description given of the Kelts by the Latin historians, stands in marked contrast to the small-limbed, light-complexioned Kelt of certain districts in Ireland, whose skin is freckled rather than burnt red by the sun. The determination of the several racial elements in these islands is particularly difficult on account of the intermixture of population, and nowhere is the difficulty greater than in the case of the Keltic portion of the community. Long before the Roman conquest the intrusive Aryan Kelt had been intermarrying with the older inhabitants of the country, who doubtless belonged to more than one race, the result being that the so-called Keltic race is an amalgamation of races differing physiologically but dominated by a common moral and intellectual character—the consequence of subjection for a long series of generations to the same conditions of life. It has become a commonplace of ethnology that the so-called Keltic race includes not only the fair-complexioned Aryan Kelt, but also the 'black Kelt' or Iberian with dark skin, black hair and eyes, and small limbs. The subject, however, is much more complex than this simple division would imply. We have seen that under the 'red Kelt' are included two distinct varieties; the 'black Kelt' is equally irreducible to a single type, while the fact that the two types of 'red' and 'black' recur in the same family—my own, for example—not only indicates their long-continued intermixture, but suggests the existence of intermediate varieties. The limitations and relations of dolichocephalism and brachycephalism within the race also need further investigation. I hope that this meeting, held as it is on the borders of what is still a distinctively Keltic country, may help to settle these and similar problems.

Meanwhile I will conclude this address, which has already extended to an inordinate length, by directing your attention to two lines of evidence which have an important bearing on the question of the extent to which the Keltic element enters into the existing British population. A few years ago it was the fashion to assert that the English people were mainly Teutonic in origin, and that the older British population had been exterminated in the protracted struggle it carried on with the heathen hordes of Anglo-Saxon invaders. The statement in the 'Saxon Chronicle' was quoted that the garrison of Anderida, or Pevensey, when captured by the Saxons in A.D. 491, was all put to the sword. But it is obvious that the fact would not have been singled out for special mention had it not been exceptional,



while it is equally obvious that invaders who came by sea can hardly have brought their wives and children with them, and must have sought for both wives and slaves in the natives of the island. Mr. Coote, in his 'Romans of Britain,' and Mr. Seeböhm, in his 'English Village Community,' have pointed out the continuity of laws and customs and territorial rights between the Roman and the Saxon eras, presupposing a continuity of population, and anthropologists have insisted that the survival of early racial types in all parts of the country cannot be accounted for by the settlement of the Bretons who followed William the Conqueror, or of the Welsh who came into England when the penal laws against them were repealed by Henry VIII. But the advocates of the theory of extermination had always one argument which seemed to them unanswerable, and which indeed was the origin of their theory. The language of the Anglo-Saxons contains scarcely any words borrowed from Keltic. Such a fact was held to be inexplicable except on the hypothesis that the speakers of the Keltic dialects were all exterminated before any intercourse was possible between them and the invading Teuton.

But I think I can show that the fact admits of quite another explanation. Roman Britain was in the condition of Roman Gaul; it was a Roman province, so thoroughly Romanised indeed that before the end of the first century, according to Tacitus ('Agric.' 18-21), even the inhabitants of North Wales had adopted the Roman dress and the Roman habits of luxury. After four centuries of Roman domination it is not likely under these circumstances that the dialects of the British tribes would have resisted the encroachment of the Latin language any more than did the dialects of Gaul. The language, not only of government and law, but also of trade and military service, was Latin, while the slaves and servants who cultivated the soil were bound to understand the language of their masters. Moreover, Britain was a military colony; the natives were drafted into the army, and there perforce had to speak Latin. If Latin had not been the language of the country at the time the Romans left it, the fact would have been little short of a miracle.

That it was so is certified by more than one piece of evidence. The inscriptions which have survived from the period of the Roman occupation are numerous; with the exception of three or four Greek ones, they are all in Latin. Of a Keltic language or dialect there is no trace. When the Romans had departed, and the inhabitants of Wales and Cornwall had been cut off from intercourse with the civilised world, Latin was still the ordinary language of the mortuary texts. It is only gradually that Keltic oghams take their place by the side of the Roman characters. When St. Patrick writes a letter to the Welsh prince of Cardiganshire, addressed not only to him but to his people as well, it is in the Latin language; when St. Germanus crosses into Britain to settle a theological controversy, and leads the people to victory against the Saxon invader, he has no difficulty in being understood; and the proper names of the British leaders continue to be Roman long after the departure of the Roman legions. What clinches the matter, however, is the positive statement of Gildas, the British writer, the solitary witness who has survived to us from the dark period of heathen invasion. He asserts that the ships called 'keels' by the Saxons were called *longe naves* 'in our language' ('nostra lingua').<sup>1</sup> In the middle of the sixth century, therefore, Latin was still the language of the Kelt south of the Roman Wall. Such being the case, it is not Keltic but Latin words that we must expect to have been borrowed by Anglo-Saxon, if the British population, instead of being exterminated, lived under and by the side of their Teutonic invaders. Now these borrowed Latin words exist in plenty. They have come not only from the speech of the towns, but also from the speech of the country, proving that the country population must have used Latin like the inhabitants of the towns. In an interesting little book by Professor Earle on the Anglo-Saxon names of plants a list is given of the names of trees and vegetables that have been taken from a Latin source. Where the tree or the vegetable was one with which the invaders had not been acquainted in their original home, the name they gave to it was a Latin one, like the *cherry* or *cerasus*, the *box* or *buxus*, the *fennel* or *feniculum*, the *mallow* or *malva*, the *poppy* or *papaver*, the *radish* or

<sup>1</sup> *Hist.* 23.



*radix*. Such names they could have heard only from the serfs who tilled the ground for their new lords, not from the traders and soldiers of the cities. It is much the same when we turn to the names of agricultural implements which imply a higher order of culture than the simple plough or mattock, the name of which last, however, is itself of Keltic origin. Thus the coulter is the Latin *cutter*, the sickle is the Latin *secula*. That other agricultural implements bore Teutonic names proves merely that the Saxons and Angles were already acquainted with them before they had quitted their primitive seats.

The philological argument has thus been cut away from under the feet of the advocates of the theory of extermination, and shown to tell precisely the contrary tale. It has disappeared like the philological argument by which the theory of the origin of the Aryans in Asia was once supposed to be supported. But there still remains one difficulty in our path.

This is the fact that the languages spoken in Wales, and till recently in Cornwall, are Keltic and not Latin. If Latin had been the language of the Keltic population of southern Britain when the Romans left the island, how is it that where the Keltic population still retains a language of its own that language is Keltic? The answer to this question is to be found in history and tradition. Up to the sixth century the Teutonic invaders gained slowly but steadily upon the resisting Britons. They forced their way to the frontiers of what is now Wales, and there their further course was checked. The period when this took place is the period when Welsh literature first begins. But it begins, not in Wales, but in Strathclyde or south-western Scotland, to the north of the Roman Wall. Its first records relate to battles that took place in the neighbourhood of Carlisle. From thence its bards and heroes moved southwards into North Wales. Tradition commemorated the event as the arrival in Wales of 'Cunedda's men.' The sons of Cunedda founded the lines of princes who subsequently ruled in Wales, and the old genealogies mark the event by suddenly substituting princes with Welsh names for princes with Latin names. The rude Keltic tribes of Strathclyde came to the assistance of their more cultured brethren in the south, checking the further progress of the foreigner and imposing their domination and language upon the older population of the country. It is probable that the disappearance of Latin was further aided not only by the destruction of the cities and the increasing barbarism of the people, but also by the settlement of Irish colonies, more especially in south Wales. At all events the ruin of cities like Caerleon and Caerwent must be ascribed to Irish marauders. We can now explain why it is not only that Wales speaks Welsh and not Latin, but also why a part of the country, which, according to Professor Rhys, was mostly peopled by Gaelic tribes before the Roman conquest, speaks Cymric and not Gaelic. As for Cornish its affinities are with Breton, and since history knows of frequent intercourse between Cornwall and Brittany in the age that followed the departure of the Romans we may see in the Cornish dialect the traces of Breton influence.

The arrival of 'Cunedda's men' and the re-Keltisation of Wales lead me to the second line of evidence to which I have alluded above. The bearing of the costume of a people upon their ethnography is a matter which has been much neglected. But there are few things about which a population—more especially in an early stage of society—is so conservative as in the matter of dress. When we find the Egyptian sculptor representing the Hittites of the warm plains of Palestine clad in the snow-shoes of the mountaineer we are justified in concluding that they must have descended from the ranges of the Taurus, where the bulk of their brethren continued to live, just as the similar shoes with turned-up ends which the Turks have introduced among the upper classes of Syria, Egypt, and northern Africa point to the northern origin of the Turks themselves. Such shoes are utterly unsuited for walking in over a country covered with grass, brushwood, or even stones; they are on the contrary admirably adapted for walking on snow.

Now the dress of Keltic Gaul and of southern Britain also when the Romans first became acquainted with it was the same as the dress which 'linguistic palæontology' teaches us had been worn by the primitive Aryans in their first home. One of its chief constituents were the *braccæ*, or trousers, which accordingly



became to the Roman the symbol of the barbarian. We learn, however, from sculptures and other works of art that before the retirement of the Romans from the northern part of Europe they had adopted this article of clothing, at all events during the winter months. That the natives of southern Britain continued to wear it after their separation from Rome is clear from a statement of Gildas ('Hist.' 19) in which he refers in no flattering terms to the kilt of the Pict and the Scot. Yet from within a century after the time of Gildas there are indications that the northern kilt which he regards as so strange and curious had become the common garb of Wales. When we come down to the twelfth century we find that it is the national costume. Giraldus Cambrensis gives us a description of the Welsh dress in his own time, from which we learn that it consisted simply of a tunic and plaid. It was not until the age of the Tudors, according to Llyd, the Welsh historian of the reign of Elizabeth, that the Welsh exchanged their own for the English dress.<sup>1</sup> The Welsh who served in the army of Edward II. at Bannockburn were remarked even by the Lowland Scotch for the scantiness of their attire,<sup>2</sup> and we have evidence that it was the same a century later.<sup>3</sup> If we turn to Ireland we find that in the days of Spencer, and later, the national costume of the Irish was the same as that of the Welsh and the Highland Scotch. The knee-breeches and sword-coat which characterise the typical Irishman in the comic papers are survivals of the dress worn by the English at the time when it was adopted in Ireland.

The Highland dress, therefore, was once worn not only in the Scotch Highlands and in Ireland, but also in Wales. It characterised the Keltic parts of Britain with the exception of Cornwall and Devonshire. Yet we have seen that up to the middle of the sixth century, at the period when Latin was still the language of the fellow-countrymen of Gildas, and when 'Cunedda's men' had not as yet imposed their domination upon Wales, the old Keltic dress with trousers must have been the one in common use. Now we can easily understand how a dress of the kind could have been replaced by the kilt in warm countries like Italy and Greece; what is not easily conceivable is that such a dress could have been replaced by the kilt in the cold regions of the north. In warm climates a lighter form of clothing is readily adopted; in cold climates the converse is the case.

I see, consequently, but one solution of the problem before us. On the one hand, there was the distinctive Keltic dress of the Roman age, which was the same as the dress of the primitive Aryan, and was worn alike by the Kelts of Gaul and Britain and the Teutons of Germany; on the other hand, there was the scantier and colder dress which originally characterised the coldest part of Britain, and subsequently mediæval Wales also. Must we not infer, in the first place, that the aboriginal population of Caledonia and Ireland was not Keltic—or at least not Aryan Keltic—and, secondly, that the dominant class in Wales after the sixth century came from that northern portion of the island where the kilt was worn? Both inferences, at all events, agree with the conclusions which ethnologists and historians have arrived at upon other grounds.

Perhaps what I have been saying will show that even a subject like the history of dress will yield more results to ethnological study than is usually supposed. It will be another illustration of the fact that the student of humanity cannot afford to neglect any department of research which has to do with the life of man, however widely removed it may seem to be from science and scientific methods of enquiry. 'Homo sum; humani nihil a me alienum puto.'

<sup>1</sup> *The Breviary of Brytaine*, Twyne's translation, p. 35 (ed. 1573).

<sup>2</sup> Barbour's *Bruce*, ix. 600-603.

<sup>3</sup> See Jones, *History of the County of Brecknock*, vol. i. p. 283; comp. *Archæologia Cambrensis*, 5th ser. No. 7 (1885), p. 227.









